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THE EXPERIMENTAL DEVELOPMENT OF COLOR-TONE SYNESTHESIA

BY T. H. HOWELLS

University of Colorado

The quality which has traditionally differentiated the psychologist from the unspoiled layman is his persistent endeavor to dissect experience. The classical psychology has commonly taken for granted that experience is an incidental or secondary product of varying combinations of discrete and independent sensations. On the other hand, the more modern, instrumental view tends increasingly to interpret all sensations and perceptions as incidental to the overall process of organic adjustment or achievement, and is inclined to suspect that all of the qualities and meanings of sensations and perceptions, the very origin, maintenance, and mode of discrimination of sensory components, depend in the long run upon how effectively they guide the total process of integration of the whole organism.

The immediate world of the child is evidently quite unitary and undivided. Neither the child nor the animal is adept at analyzing his experience. On the other hand, the more sophisticated psychologist tends characteristically to departmentalize and classify his impressions of his environment. The first experimental psychologist was perchance some primordial boy who lay idly on his back and wondered at the marvelous changes which could be wrought in Nature, simply by opening and closing his eyes. As psychologists have done ever since, he perhaps conjectured that the complex experience which came and went as his eyelids moved was furnished solely by his eyes—in other words, the *erreur de préjuge*. He was probably quite naively certain that the peculiar shivery quality of the sight of white snow, for instance, was altogether optical, and ignored the possibility that many of its qualities were originally derived from his other senses. The boy saw the snow as cold and wet because very likely he had once felt that it was cold and wet at the

same time that he saw that it was white. Also, he now saw it at a distance because when formerly he saw and felt it, he had also reached or walked to it, and thus added kinesthetic data to his perceptual composite. If one hears the sound of wheels crunching the cold snow outside, he may be unable to restrain a shiver, even if he is sitting by the fireside, especially if at some time or other he has happened to hear this peculiar squealing noise when he also suffered with the cold and probably struggled to reach warmth. All of these transfer effects are probably dependent, not only upon simultaneity of different stimuli, but also upon their utilization or involvement in a purposive quest for some sort of end, in other words, in an operant response. Recent developments in psychology are challenging us to consider the hypothesis that all our observations of the world are incidental to what we are doing at the time, to our larger and longer program of adjustment or goal achievement, such as trying to obtain food or shelter, and through them, perhaps, survival.

Linkage of the senses, so that stimuli normally reacted to by only one kind of receptor may produce experiences or reactions which are supposedly specific to another sense, is commonly known as synesthesia. It is evidently involved in all perception to a much greater extent than is generally realized. As a matter of fact, if perception is regarded as a synthesis of more elementary impressions, synesthesia may be considered as practically identical with perception. Synesthetic processes may accomplish the basic functional integration of sensory data, not only between the different senses, as usually assumed, but also within a single sense, such as hearing or seeing. For instance, a photograph of a landscape on a book page gives the effect of seeing far distance, even though the accommodation of the crystalline lens and the convergence of the eyes, which are normally the principal means of perceiving distance, are adjusted for close-up rather than for far vision. In spite of this interference, perception of distance in a picture is nevertheless achieved vicariously because of a preponderance of other associated factors supplied by the photograph, and which usually accompany the normal ocular adjustment for seeing distance, such as differences in sizes of the retinal images of far and near objects, perspective, contrast, brilliancy, superposition, etc. The depth effect in a picture is improved if one eye is closed so as to get rid of contradictory evidence from that eye indicating that the picture is a flat surface.

Errors in perception of distances when one first wears a new pair of glasses are probably a result of an influence which may be termed synesthetic. The changed refraction of the new lens imposes a different ocular accommodation from that formerly used for focusing a given object, and synesthetically perverts the evidence of the other

factors, which results in a misjudgment of distance. Errors in spatial orientation when using the pseudophone or pseudoscope might also be classed as synesthetic, since in each instance sensory cues (auditory, visual, and kinesthetic) are working in conflict with each other; the influence of any one of these on the others might very well be interpreted as synesthetic. Ventriloquism is evidentally a synesthetic phenomenon too, since both the source and quality of the sounds are misinterpreted because of more influential visual evidence supplied by the puppet but concealed by the ventriloquist.

In G. M. Stratton's famous experiment on inverted vision (**6**), the interference between visual and auditory cues in locating objects in space is evidence of a synesthetic influence of each on the other, since the effect of sound was to change visual perceptions. The fact that this interference was afterwards overcome is evidence that in the long run perceptions are subsidiary to the total process of adjustment.

In these instances, as always, temporary conflict between ordinarily integrated factors is necessary to demonstrate their normal coördination and mutual support. Such coöperation between and within the senses is obviously indispensable to the organism in attaining its objectives, and serious interferences between them may result in organic disaster. As long as sensory data coöperatively contribute to organic adjustment their existence as elements is ignored. Recognition occurs only if and when they are removed, or else contradict the meaning of each other and thereby produce intra-organic conflict.

A very interesting and obviously spontaneous development of synesthesia recently came to the attention of the writer. For some years an auto driver had frequently passed a certain street crossing where a whistle was sounded automatically whenever the lights changed for *stop* and *go*. It happened later that the whistle was discontinued, although the lights continued to change as usual. A few days after this discontinuance, when a friend remarked to the driver that the whistle no longer sounded, he vigorously denied it and claimed he had heard the whistle that morning. On the next trip to the crossing, in spite of counter suggestions recently received, the driver reported a distinct hallucination of the whistle. This effect gradually diminished when further investigation proved that the whistle no longer sounded. Even the pitch of the tonal image, or hallucination, as reported by the driver when the lights changed, was identified by him on an adjustable whistle and later verified as approximately correct by comparison with the actual whistle. Other tests made away from the vicinity of the lights indicated that in a different setting the driver apparently could not so exactly reproduce this tone from memory.

In another instance reported to the writer a higher-toned whistle was normally sounded when the lights changed to red for *stop* and a lower tone accompanied the *go*. Few drivers noticed that there was a difference in the tones, or even that a whistle sounded at all, until the wires controlling the whistles were accidentally crossed by a repairman, so that the usual combinations of color and tone were reversed. The result was general confusion and a collision at the crossing.

As did the deceptive instances reported, the standard psychological illusions generally arise in a conflict situation and are usually temporary. They are often overcome by a process of counter-conditioning, in which new meanings and values are afterwards ascribed to the contributory sensory cues as a consequence of a general reorganization of the activities which they mediate, and finally because of influence of the overall process of organic adjustment which is itself mediated in turn by these activities.

Problem

Consideration of reports of phenomena, such as those described above, resulted in the formation of a general hypothesis of synesthesia: namely, that synesthetic effects may be the result of an association or combination of sensori-motor activities, if and when they are functionally involved in a larger and longer process of organic adjustment or goal achievement, or in other words if they are derived from a dynamic whole. This hypothesis seemed to offer a somewhat novel interpretation of synesthesia, and also suggested a new and untried method of producing it experimentally. Also this new experimental approach seemed to provide a possible medium for testing, or at least application of, the general hypothesis that all sensory experience, including synesthetic, is instrumentally determined.

We therefore cast about for a specific experimental situation which might provide the basic conditions for such a test: namely, first, that different receptors of the subjects would be simultaneously stimulated by appropriate controlled stimuli; secondly, that the sensory functions involved must normally work together coöperatively in achieving a psychological end or goal, so that this end becomes the chief object of attention rather than the sensations themselves; and, thirdly, that an unexpected conflict between these normally integrated components should be introduced occasionally as a means of obtaining objective evidence of their normal synesthetic integration.

On first consideration it may appear that our problem was very similar to that of others who in times past have attempted, rather unsuccessfully, to produce synesthetic effects through conditioning.

The major attempt to develop synesthesia through conditioning was apparently that of E. L. Kelly (1), who presented certain organ tones in combination with certain colored lights to a number of subjects. His test for conditioned synesthesia was an introspective report from his subjects that they either did or did not experience the corresponding color sensation when a given tone was sounded alone, or else that they heard the proper tone when the paired color-stimulus was presented by itself. Although these paired presentations were apparently continued for a long enough time to achieve positive results if they were at all possible, no success was obtained.

Comparison of the experimental means or methods of Kelly's experiment with those outlined above shows that while he did present certain combinations of visual and auditory stimuli, they were not used to achieve any end beyond themselves. Though his technique satisfied our first experimental requirement, it did not satisfy the second. Also his data were introspective reports from his *S*s, while ours were errors in discrimination resulting from an experimentally-produced conflict between normally corroborative perceptual cues; his experimental criteria were subjective while those of this experiment are objective. Therefore, Kelly's set-up did not satisfy the third condition specified above. His *S*s merely looked and listened because nothing else was necessary or demanded of them. It should be remembered that in the set-up of the present experiment the tones and colors involved constituted the necessary means for making difficult discriminations, and that success or failure in these discriminations meant success or failure in the work the *S* was paid to do. Our *S*s did not try to achieve the results that meant success of the experiment; rather they tried to avoid them.

Method and Materials

The general method of the present experiment was to require the *S*s to identify the color of a stimulus patch as either one or the other of two complementary colors, under circumstances in which the colors might vary from a maximum to a minimum saturation of either, that is they might be either strong or weak samples of two different hues. Identification of the colors was ordinarily facilitated by paired tones which were sounded before and while the colors were being shown, so that each color was normally indicated before it was seen by a distinct, characteristic tone. It is evident, therefore, that in spite of diminished or pale tints, discrimination between the colors would have been easy and certain if the tones were always dependable indicators. At irregular intervals, however, the indicator tone was a wrong or false one, influencing the *S* to make mistakes in his judgments when the discrimination was difficult, especially if the normal tone-color association was strong.

It is evident, therefore, that sight and sound stimuli ordinarily supplemented and reinforced each other in determining choices, but occasionally were in conflict, so that as the strength of the normal associations increased, interference and confusion increased for the reversed combinations. The effect was to motivate the *S*s to pay ever closer attention to the stimuli, because making mistakes supposedly incurred a financial penalty. Consequently the *S*s could not be passive toward the stimuli, but were obliged to use them as efficiently as possible in order to achieve a desired end. Since in the great majority of instances (95 percent of the practices)

the customary combination of color and tone occurred, that is, the indicator tone was correct, the effect of additional practice, in spite of errors, was to establish the normal associative connection ever more firmly, thus setting up a 'vicious circle' tending constantly to increase the susceptibility of the *Ss* for making errors as practice increased. For this reason, evidently, errors should constitute good evidence of sensory conditioning—which had developed in spite of the efforts of the *Ss* to avoid it.

The *Ss* of the experiment were eight university students who were paid for their services by the National Youth Administration. None of them had taken courses in psychology, and they were kept as ignorant as possible of the purposes and possible developments of the experiment. All of the *Ss* passed the Ishihara test for color vision, and all achieved better than the 40 percentile position on the Seashore test for pitch discrimination. They worked together in pairs, one student serving as *S* or observer while the other served as experimenter, giving directions, operating the apparatus, and keeping records. Every 20 min. they exchanged functions in order to avoid fatigue.

The experimental procedure was carried out in a dark room. The *S* was seated facing the screen of a color mixer, or chromatoscope, at a distance of six feet. The chromatoscope was especially designed and constructed for the purpose of this experiment. The screen was 13 cm. horizontally by 19 cm. vertically. It was translucent and was lighted from inside the mixer. The color could be readily and reliably varied from one extreme of saturation of a given hue to another hue, through an intermediate continuum of blends, all the while maintaining approximately the same brilliance. The intensity of illumination of the screen was approximately two foot-candles. When two complementary colors were used, as in this experiment, blending one color with another tended to reduce its saturation until, with an equal mixture of the two colors, a bright, neutral gray or white was produced. The colored light was supplied by two spotlight projectors mounted near opposite corners of one side of a light-tight box and supplied with appropriate filters to color the light. The spots were projected across the blackened interior of the box to a white screen on the opposite side, and thence the light was reflected back across the box to the translucent screen which was located between the spotlights. This reflection very effectively mixed and diffused the colored lights so that a very even blend was produced on the screen. The color stimulus could be varied from one extreme to the other by operating a slide to which shutters were attached, which reciprocally increased the light from the green spotlight while that from the red one was diminished, and vice versa. An indicating pointer was attached to the slide so that various blends could be identified on a scale. Thus it was possible to reproduce them at will, and also to measure and compare them.

Procedure and Explanation

The *S* was shown the screen of the chromatoscope, and then the lights were turned off and he was asked to close his eyes. He was then told that presently he would hear a musical tone, whereupon he was to open his eyes and note the color of the screen, report it as either *red* or *green*, and then close his eyes again.

The red color was actually carmine, and the green was a bluish green, so that the colors were approximately complementary, but for the sake of convenience they were referred to as *red* and *green* during the course of the experiment and the practice will be continued in this report. The tones were produced from organ pipes which were blown by the *E* for approximately a two-sec. interval. A lower tone, having a vibration rate of about 261, or middle C, characteristically accompanied the showing of red. The higher tone, usually accompanying green, was approximately the G above, or about 392 vibrations per second.

The chromatoscope was switched on just after the tone was sounded and both stimuli were continued for approximately two sec. and stopped at the same instant, which meant that the color stimulus was observed for slightly longer than one sec. Except for the lighted screen, the room was completely dark during experimentation.

A predetermined random order was followed in presentation of red or green stimuli. This fixed order, running through a series of 20 before repetition, was indicated to the *E* by a pattern of tacks driven in the edge of a writing board, and was read by sense of touch in much the same way that Braille letters are read by the blind. A number of these boards, indicating differens random orders, were used, so it was impossible to anticipate any particular order. As soon at the stimuli ceased the *S* reported the color, closed his eyes, and waited for the next trial. The

E immediately reported success or failure to the *S* by the words *right* or *wrong*, which record was simultaneously checked on a paper data sheet attached to the board on which the random order of trials was indicated. A rest interval of about five sec. elapsed after one trial was finished before the next began.

A new *S* was given five practice trials, which included the presentation of all combinations of colors and tones. Following these he was told: (1) that there would be a long series of such trials; (2) that the showing of red and green would be in irregular or random order; (3) that in some cases the color would be more intense than in others; (4) that while certain tones would usually precede certain colors, this would not always be the case and that he should be on his guard for exceptions in order to avoid making mistakes in his judgments; and (5) that his success in his work, and perhaps in keeping his job, depended upon making as few mistakes as possible.

After the introductory period the *S*s worked together in pairs, alternating in the capacity of *E* and *S* approximately every 20 min., usually over a two- to three-hour period.

As mentioned previously, showing the red color was commonly preceded by sounding of the lower tone, C, while green was preceded by the higher tone, G. Also these combinations of tone and color were reversed, that is the indicator tone was wrong, in five percent of the trials, or one time in 20, on the average. A chance order was arranged for these exceptions by whirling a miniature roulette wheel with 20 numbers on it and recording a number so obtained on each data sheet, which provided numbered spaces for the records. When this data sheet was attached to one of the writing boards, an extra thumb tack was placed opposite this practice number, as indicated by the tacks on the edge of the board. This arrangement indicated tactually to the *E* when the different or misleading indicator tone should be sounded. Thus it might come anywhere at random in a series of 20 practices, from the first to the twentieth, and, therefore, the *S* never knew when to expect a misleading tone.

The procedure as outlined, using all color stimuli of maximum saturation, was carried out for 5000 practices for each *S*. It occupied about 12 hours working time for the two students working together. During this period the *S*s did not have much difficulty in recognizing the color of the light stimulus when they looked at it. Because of the influence of the tone which characteristically preceded it, however, there was a growing temptation to prejudge it, and to get ready to give it the customary name. One might say that a habitual tendency was developed to make the same verbal response to the conditioned tone stimulus which was previously given to the color stimulus. Except for the reversals, in which the tone was a false indicator, it would of course have been feasible for the *S* to make his judgments with his eyes closed. The continuous possibility that the tone might be misleading, however, made it necessary for the *S* to pay critical attention to each color stimulus, which of course had the effect of establishing more and more firmly the associational connection between a given color stimulus and its usually-associated tone.

That this was the case was indicated by the fact that the *S*s would sometimes start to name the color indicated by the tone and then change their judgment, because, as they said, "it dawned" on them that the color was not the expected one. In such instances some *S*s reported that, on momentary first glance, the color actually seemed to be that indicated by the tone, but that in the process of visual perception, or 'dawning,' the hue seemed to change while they were looking at it, indicating not only that the mere verbal response was conditioned, but also that the visual manner of perceiving the color was also similarly conditioned. This tendency became so pronounced, especially during the later trials (after the first 5000), that four *S*s spontaneously developed a tendency to delay naming the color for a second or two after the stimuli had stopped because their color perception seemed to 'clear up and become more definite' by that time.

Fig. 1 presents graphically the relationship between errors and the number of practices, and also the number of *tests*, as reversals of the customary combinations of tones and colors were called. The number of errors shown by points on the graph is the number made during a period of 1000 practices or 50 tests. Practically all of the errors occurred on the tests and no others were counted. As will be noted, very few errors were made by any of the *S*s during the first 5000 practices and most of these were probably accidental, that is were slips of the tongue rather than false perceptions of color.

After a given *S* had completed 5000 practices, and had been given 250 test reversals of tones, he was continued as before, except that the saturation of the color, or admixture of hue, in the light stimuli was greatly reduced on every other practice, that is, in half of the practices. This meant of course that half of the reversed or test trials would also be made with these pale-color stimuli.

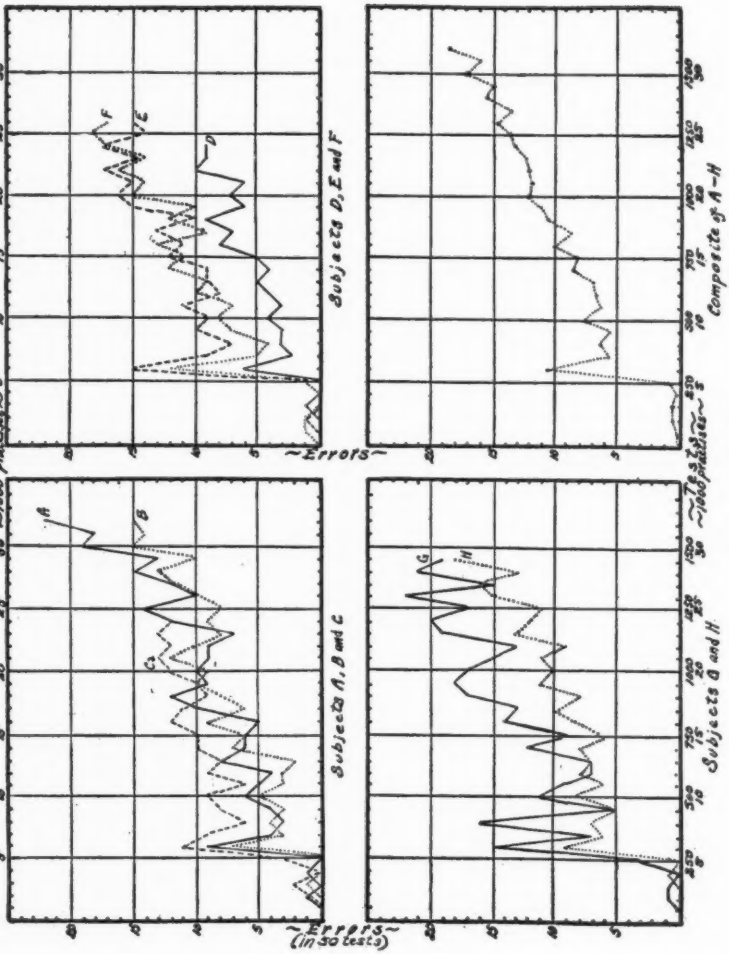

FIG. 1. Error curves.

For the first two *Ss*, numbers A and B, this reduction was achieved somewhat gradually and the degree of saturation was readjusted during a short preliminary period.[1] All of the other *Ss* were shifted to the standard pale-color stimuli on every other practice as soon as they had completed their preliminary training period with the saturated colors.

There was no expectation that a tone could ever be sufficiently conditioned as a color stimulus to counteract or reverse the perceptual effect of a color as highly saturated as the one by which it was originally conditioned. It was believed possible, however, that such a tonal stimulus might be sufficiently conditioned to reverse the perception of a pale, unsaturated color stimulus. The purpose in using only colors of reduced saturation on every other practice, and saturated colors on the other practices, following the preliminary period, was to avoid extinction of any conditioning already developed, and to reinforce it if possible. A certain amount of reinforcement was perhaps obtained from the paler colors but probably not a great deal. The reason for not using a larger proportion of practices involving saturated colors, and for not confining the tests to pale-color practices only, was that such limitation would have enabled the *Ss* mentally to exclude the possibility of making mistakes during the saturated-color practices, and also would have eliminated the motivation necessary to make conditioning effective during these trials.

Results and Interpretation

Fig. 1 shows the error curves for the eight *Ss* individually, and also a composite of these, as the practices continued beyond the preliminary period.

It will be noted, first of all, that immediately after the introduction of the less-saturated stimulus colors, all of the *Ss* showed a sudden increase in errors, and that for five of the eight *Ss* this increase was considerable. Secondly, it will be noted that after this initial debacle of errors, made chiefly on the first 50 tests after introduction of the paler colors, there was a tendency toward recovery during which period errors were reduced.

The reason for the sudden increase in errors is perhaps evident. After the *Ss* had become accustomed to the more intense colors, the paler colors seemed by contrast relatively indeterminate and lacking in color character of any kind. The *Ss* became disoriented in their color judgments and, of necessity, were compelled to rely very largely upon the indicator tones, which were of course misleading in the test trials. The *Ss* very naturally knew that test trials were interspersed with the others, and their suspicion undoubtedly persuaded them to disagree with the misleading indicator tones on some of the tests, which perhaps reduced recorded errors to some extent, but it also led them to disagree when the indicator tones were right, and therefore to make errors which do not show in the curves. For these

[1] The large number of errors made when the pale stimuli were first introduced led us to suspect that the task of discrimination had been made too difficult. A trial with new, unconditioned *Ss*, however, showed that it was not too difficult for them, so the saturation was permanently standardized at near the value we had originally selected. This was conveniently accomplished by attaching hinged stops which limited the range of movement of the slide on the chromatoscope whenever they were in place. We had no available means of measuring the admixture of hue in these low-saturation colors. It was very diluted or pale, but was readily recognized and named as *reddish* or *greenish* by five unconditioned persons who served as controls.

reasons, therefore, the curves probably do not represent the actual extent of failure to discriminate the colors.

The *S*s generally reported a general lack of confidence in their judgments when the paler colors were introduced. In some of the tests, however, they reported more confidence than in others, and apparently their judgments on these tests were more often correct. For some reason, perhaps involving momentary distraction of attention, disorientation, or some other accidental factor, the indicator tones were much more disturbing for some tests than in others, and the perceptions of color were more uncertain in some practices than in others. On the whole, however, the number of errors in judgments seemed to constitute a good measure of actual perceptual interference.

The explanation of the second phase of the error curve, during which there was temporary recovery and errors diminished, is perhaps harder to find. Apparently the *S*s became adjusted to some extent to the new situation and gradually revised their previous perceptual norms for color comparison, or perhaps developed new ones. Some of them reported that they "learned what to look for," which meant perhaps that they had learned to concentrate attention on significant cues.

The third, and perhaps the most interesting, aspect of the error curve is that after the sudden increase in errors, and subsequent decrease showing recovery of discrimination by the *S*s, the curve begins to rise again with further practice and errors increase again over a considerable period. The only possible explanation of this continuous increase in errors seems to be that it is evidence of cumulative conditioning by which the substitute tonal stimulus was gradually strengthened by further association with the stronger or saturated color-stimuli, until it was able to dominate and reverse perception of the weaker color-stimulus in an increasing proportion of the tests. The habitual set, or expectancy of seeing a given hue after hearing the paired tone, became so strong that it overpowered the conflicting perceptual influence of the hue actually supplied, with the result that pale green was actually seen as pale red, or vice versa.

At least, this was the explanation offered by the *S*s. It is possible that some of them had learned or surmised that such effects were to be expected in the course of the experiment, but it is not probable that they knew enough about it to anticipate developments. As mentioned previously, they were freshmen and had not taken courses in psychology. It should be understood also that they were not motivated to make such mistakes in perception, but rather to avoid them.

A possible fourth aspect of the composite curve, in so far as the part during which the unsaturated colors were shown (the latter or main part) is concerned, is that except for the initial decline it is

approximately a straight-line curve. The sudden rise in the curve during the first part of this period is probably best understood as an aftermath of the sudden shift from the saturated to the unsaturated colors. It would have been worth while, certainly, to have continued the practices for some time in order to develop the upper part of the curve, but limitations in available time of the *S*s made this impossible.

General observation of the behavior of the *S*s near the end of their period of service indicated to the writer that their errors probably would have continued to increase, perhaps at an increased rate, if they had continued with the practices. They seemed to develop nervousness as their errors increased, and showed a disinclination to go on with the experiment. Two out of the eight (A and G), at least, became uncertain and erratic in making their decisions and changed them so often that it was difficult to record them with certainty. One or two of the others seemed to become indifferent and apparently made decisions more or less at random without much attention to the colors. One of the *S*s (A) showed symptoms of incipient behavioral disorganization, failed to keep appointments, and tried to transfer to another job at a sacrifice of regularity and convenience in hours. Although most of the *S*s said that they preferred to act as observers during the first part of the experiment, they all preferred the job of serving as experimenter during the latter part of it. They reported that sitting as an observer was more tiresome, and also that it made them 'jumpy.'

Actually the impression of the writer was that unwittingly the experiment had achieved about as much success in developing neurosis as in creating synesthesia. As a matter of fact, it is quite evident that the procedure necessarily developed a conflict between opposed perceptual tendencies and resultant choices which greatly resembles the conflict situation of Pavlov's dogs (**5**), obliged to discriminate increasingly similar geometric patterns; of Maier's neurotic rats (**4**); and of other victims of experimental or accidental neurosis. As the strength of the usual combinations of tone and color were strengthened, the reversed combinations were increasingly in conflict. This was, of course, the planned set-up, which was intentionally responsible for the increase in errors with additional practice.

Testing Validity of Evidence

There is perhaps some justification for considering whether or not the conditioned response to the tonal stimulus was not primarily a verbal response rather than a perceptual response; that is, that the *S*s were directly conditioned to say the word *red*, for instance, rather than that the word was a report of what they actually saw. There is

no reason to doubt that conditioning did facilitate saying the words most frequently associated with the tones. The *S*s did show a tendency at times toward speaking the associated word thoughtlessly or mechanically as soon as the tone was sounded. It is very probable that this tendency was responsible for some of the errors, especially in the early part of the experiment. The automatic corrective for such impulsive responses, however, was the fact that they resulted in making mistakes which the *S*s wanted very much to avoid. They soon learned to be on their guard against making such rash judgments, and, as previously mentioned, four of them spontaneously developed the practice of waiting for a brief time after both stimuli had ceased before making their judgments, partly at least because they believed that the color image cleared up and became more dependable by that time. The manner of the *S*s in making their verbal reports also tended to give the impression that in most instances they were reports of actual visual observations rather than mere mechanical vocalizations.

Still, it cannot be denied that the conditioned verbal response was actually a part of the total organismic response, and undoubtedly contributed its share in determining decisions about color. One of the *S*s reported that, even after deliberate delay of his report, the word indicated by the tone still "trembled on the tip of my tongue." Perhaps the best interpretation is that the verbal response was as much a means by which the perception was mediated as was the word a resultant of the perception.

In the hope of testing the hypothesis that the supposed synesthetic effect was merely a conditioned verbal response to a tonal stimulus, that the supposed distortions of color perception were perhaps merely slips of the tongue, an additional test was devised and given to two of the *S*s (G and H) when they had nearly finished their part in the experiment.

They were trained to use an extension rod to operate the slide on the chromatoscope so as to adjust it for what appeared to them to be a point neutral between the two colors, or the best possible white, moving in on one trial from bright red toward the green, and on the next from bright green toward the red, in alternating order. After each adjustment was made, a reading of the scale of the chromatoscope was taken. When the *S*s had attained some degree of efficiency, the exercise was continued as before, but now one or the other of the tones to which they had previously been conditioned was sounded quite continuously all the while they were adjusting the chromatoscope for white. A scale reading for such adjustment was made and recorded, and then another adjustment was made starting with the other color, and perhaps with the other tone sounding.

XUM

Adjustments were made alternately, starting with different colors, but the high and low tones were sounded in random order. One hundred readings were made for each of the two *S*s .

Frequency distributions of readings of the scale of the chromatoscope were made, one while the lower tone, C, was sounded, and one while the higher tone, G, was sounded. The mean scale value for 100 combined readings with the lower (red) tone was 4.64, and was displaced from the point of adjustment for white (5.00) toward the green, indicating apparently that additional green was needed to compensate for the coloring effect of the lower (red) tone. The mean scale value for 100 readings with the higher (green) tone sounding was 5.42. The difference between the means was .78, and the standard error of the difference in the means was .19, affording a critical ratio of 4.1. This seems to constitute reasonable proof that the tones actually exerted a coloring influence upon the visual perceptions, independently of any verbal response.

Another possible explanation of the increase in errors as the *S*s continued with the experiment is that they made more errors because they had lost interest in their work and did not make the effort to avoid errors that they did earlier. Perhaps the threat of losing their job became less effective, since they were nearing the end of their employment period. In other words, it might be suspected that errors in discrimination of the colors would have increased regardless of whether or not the tones accompanied the showing of them.

While it must be admitted that the work was routine and long drawn out, and perhaps monotonous, there is no reason to conclude that it was more tedious than the usual drudgery of NYA students. As mentioned, the *S*s alternated in the roles of *E* and *S* every 20 min., which afforded considerable relief from monotony. Also it was natural that the two *S*s working together should compare their scores quite frequently, and this seemed to inject an element of competition between them which added interest to their work. On the other hand, it was unavoidable that they should recognize that their errors were increasing rather than diminishing and this may have contributed to a sense of failure, defeatism, and also to the nervousness previously mentioned. Nervousness, however, was evidently more of a result than a cause of increase in errors. It is possible that the threat of discharge lost effectiveness with time. Still it was known that other NYA students had lost their employment because faculty supervisors had reported them as unreliable or inefficient. The NYA employees generally showed a sense of obligation and responsibility and were hopeful of future employment if they were successful with their present work.

In order to test the hypothesis that errors had increased because of loss of motivation, and irrespective of the influence of the tones, the following procedure was carried out with four *S*s (E, F, G, and H).

At or near the end of their practices with the tones, they were asked to go through exactly the same routine, except that the *E* substituted the word *now* for the tone as a signal for the *S* to open his eyes and make a judgment of the color stimulus. It will be noted that the same word was used for both colors and therefore could provide no preliminary clue as to what the color would be. The same random method of selecting the test trials on which the customary tones would have been reversed, if they had been used, was carried out. Only errors of discrimination on these trials were counted. Each *S* completed 1000 practices, including 50 test trials, in this situation. Subject E made 5 errors, F made 3 errors, G made 6 errors, and H made 3 errors. The number of errors without the tones was less than one-fourth the number they had made just previously, during an equal number of trials with the tones, which would seem to demonstrate that the tones actually did contribute toward the increase in errors.

Two of the *S*s, G and H, immediately after completing the test just described, were individually given 100 trials with the arrangement that every other trial presented one or the other of the colors of maximum saturation at random. It was preceded and accompanied by the usual tones, as was the customary practice in order to reinforce conditioning during the period of increasing errors. On the other or alternate trials, however, in which pale or unsaturated colors had previously been used, a neutral or best-white stimulus was used instead. This neutral color was preceded and accompanied, as were the unsaturated stimuli, by the high and low tones in random order. These tests were administered by the writer, so that neither of the *S*s anticipated the nature of the stimuli or purpose of the tests. In every instance the reported color of the white stimulus was that indicated by the tone. One of the *S*s afterwards informed the writer that he suspected during the course of the tests that the pale stimuli were actually white, but that nevertheless they really appeared colored to him. The other *S* said that it did not occur to him that the stimuli were white.

The latter *S* (G) made an interesting comment that in one of the trials described above the tone was accidentally discontinued before the chromatoscope lights were turned off, and that on cessation of the tone the light stimulus changed from red (the color induced by the tone) to green, rather than to its actual white. He was tried out again in this situation and apparently verified his observation. If the effect is genuine, it would seem to mean that the laws of color

contrast, perhaps as relating to after-images, do not depend always upon specific physical stimuli and may be psychological as well as physiological. The same *S* also volunteered that he saw a clear image of the screen of the chromatoscope, and of the color normally associated with a given tone, when this tone was sounded while his eyes were closed or in the darkened room. He could get an after-image of the lighted rectangle which was of a color complementary to the actual color, but his after-images, when the rectangle was psychologically colored from the effect of a tone, were dark only with no color noticeable.

Summary and Conclusions

A low tone was sounded before and during the showing of a reddish light, and a higher tone was similarly associated with a bluish-green light, for 5000 presentations to eight *S*s, who reported the color of the seen stimulus on each presentation. They were motivated to pay close attention to the color, and not to depend upon the accompanying tone, by occasional reversals of the usual tone-color combinations. These reversals influenced them to make mistakes in their reports, for which, supposedly, they might lose their jobs. Thus, the attitude of the *S*s toward the stimuli was active or purposive, rather than passive. After completing these practices, unsaturated color stimuli, pale but recognizable, were substituted on alternate practices, and for one half of the reversed or test combinations. When the tones were thus switched, many mistakes were made in identifying the hues of these pale colors. The same combinations of stimuli were continued with these *S*s for about 20,000 additional presentations, involving 1000 interspersed test reversals of tone. Although there was a temporary recovery from earlier errors, the proportion of errors continued to increase with additional practice.

The errors may be interpreted as an effect of synesthesia which was developed through conditioning. This interpretation is borne out by evidence that the errors were not due to diminished interest or effort, and that they involved an actual distortion of visual perceptions rather than errors in verbal responses only, or mere slips of the tongue.

Whether or not the developments of this experiment are representative of true synesthesia, or merely what one writer has called pseudo-synesthesia, must be left to the personal judgment of the reader. It has sometimes been argued that real synesthesia is always inherited. If this definition is accepted, then of course the conditioned linkages of stimuli and responses, presently reported, are not synesthesia. Also, if it is presumed that perception is purely an affective phenomenon, to be observed only by introspection, and not

dependent upon motor responses of any kind, then the present evidence is not pertinent with respect to the problem of synestheisa.

Many of the traditional cases of synesthesia have exhibited very extreme and novel complications. Some of the sensory combinations reported in modern scientific literature have also been quite sensational. They are reviewed by Langfeld (**2**, **3**). The color, tone, number, and letter associations of the blind, particularly, are sometimes very complex and intricate in structure. The obsessional character of some, and marked elaboration of many, of these combinations suggest that they may have a compensatory or symbolical significance for the *Ss* similar to that of the graphic and vocal elaborations characteristic of various regressional disorders.

It is very probable that certain types of unadaptive personality might become disorganized sooner and make more errors than others in this present experimental situation. The behavior of some of our *Ss* suggested this possibility. Rigid inflexibility of associations in the earlier practices would evidently contribute toward such later disorganization. Innate differences also probably culminate in later differences in the dynamic balance of a personality. It is possible that the present technique might be adapted to produce experimental neurosis, or perhaps to diagnose or predict incipient neurotic tendencies. Some of our *Ss* showed more neurotic symptoms than others in the experimental conflict situation.

The general or theoretical implications of this experiment might be listed as follows:

1. That tonal stimuli can be conditioned to influence judgments of color stimuli in a manner which is apparently synesthetic.

2. That the mode of purposive use of sensory stimuli influences the possibility of conditioning their perceptual and adjustive responses, since the present experiment, unique in providing interdependency between sensory data in guiding organic adjustments, was successful in influencing judgments of color by means of sound stimuli, whereas previously-reported experimental attempts were not successful.

3. That sensory or perceptual data are not inherently distinguishable from each other, or from the supposedly-independent motor adjustments which they mediate, but that such distinction is necessarily a function of an effort to resolve a conflict in a larger program of organic and biological integration. The reason sensory data have had such unquestioned, common-sense acceptance, as unconditionally and absolutely ultimate, is that in normal everyday living only certain typical variations in environmental factors occur, and that adjustment to these is effectively achieved by our normal assortment

of sensory modalities. Abnormal conditions, however, may impose striking transformations of these modalities, as the present experiment demonstrates.

The practical implication of this experiment is the evident necessity for integrating sensory data with motor functions and the possibility of supplementing the data of a given defective sense by data from another sense. The various senses obviously have much more in common than is generally realized. Such supplementation is strictly limited, however, by the possibility of differentiating and integrating the data of the substituted sense into the general adjustive pattern normally supplied by the defective sense. Defective vision, for instance, may be, and often is, supplemented, to a much greater extent than even the visually handicapped are aware, by well-organized auditory data. The therapeutic problem is to facilitate this differentiation, organization, and substitution, partly through training, and perhaps mechanically, in part. Thus it may come to pass that the blind shall see again—through their ears.

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A PRELIMINARY NEUROPHYSIOLOGICAL INVESTIGATION OF THE BEHAVIORAL CONCEPT OF STIMULUS TRACE [1]

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Introduction

The present investigation was undertaken in an attempt to discover whether or not there was some readily obtainable central neurophysiological correlate of Hull's postulated afferent receptor impulse (**8**) or, as it has also been referred to, *stimulus trace.*

According to Hull's recent formulation: "When a stimulus energy (S) impinges on a suitable receptor organ, an afferent neural impulse (s) is generated and is propagated along connected fibrous branches of nerve cells in the general direction of the effector organs, via the brain. During the continued action of the stimulus energy (S), this afferent impulse (s), after a short latency, rises quickly to a maximum of intensity, following which it gradually falls to a relatively low value as a simple decay function of the maximum. After the termination of the action of the stimulus energy (S) on the receptor, the afferent impulse (s) continues its activity in the central nervous tissue for some seconds, gradually diminishing to zero as a simple decay function of its value at the time the stimulus energy (S) ceases to act" (**8**, p. **47**).

There are, thus, two components involved in this formulation: one of these is present during the time the stimulus is acting; the other, following the termination of the stimulus.

It occurred to us that the well-known effect of the blocking of the alpha rhythm of the electroencephalogram during and following a brief stimulus might serve as a measure of central neural activity which could be correlated with either one or both of these two components. Jasper (**4**) indicated some time ago that there was a negative relationship between the intensity of a stimulus and the latent time of blocking of the alpha rhythm; he also reported a positive correlation between the duration of the blocking of alpha and intensity; the duration of the stimulus was kept constant. Cruikshank (**3**) investigated this problem fairly extensively and found a negative relationship between intensity and latent time of blocking

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with duration constant, as well as a negative relationship between duration of the stimulus (in msec.) and latent time of blocking, when intensity was constant. She also reported correlations between the duration of the block and these variables.

The data of Jasper and of Cruikshank are of the utmost value in forming a preliminary notion of possible measurable central neurophysiological correlates of the afferent neural impulse, for they make possible a first approximation of the type of function which may be obtained between a manipulable stimulus variable and certain presumably cortical effects.

Statement of Problem

The present investigation was specifically concerned with the determination of changes in the amplitude of the alpha rhythm along a temporal continuum, during and following the application of a single stimulus.

Apparatus and Procedure

Electroencephalograms were secured from the occipital area by means of a high-gain, low frequency amplifier which drove a Grass ink-writing oscillograph. The lead used was of the monopolar type. The *Ss* sat in a sound-shielded, dark room. They were instructed to keep their eyes closed at all times and not to react to the stimuli in any systematic manner, but, insofar as possible, to remain 'neutral.'

In order to avoid the effects of visual after-images (Jasper and Cruikshank (5); Travis and Hall, (10)), which prolong the recovery time of the alpha rhythm following visual stimulation, we employed an auditory stimulus. This stimulus was provided by a 180-cycle generator. The time of delivery of the stimulus, as well as its duration, was recorded on the oscillograph record by means of a second element. The intensity of the stimulus was constant in all experimental series: 40 db. The duration of the stimulus was kept constant in each experimental series. In the first group of experiments the duration was 3.0 sec.; in the second, it was 0.20 sec.

In order to provide a continuous measure, in time, of changes in the amplitude of the alpha rhythm the following method of analysis was used. In the first experimental series the total stimulus interval, 3.0 sec., was divided into six equal sub-intervals of 0.50 sec. In each of these intervals the total trough-to-peak alpha amplitude was computed (in mm.). Two 0.50 sec. intervals were marked off just prior to the delivery of the stimulus, and the trough-to-peak alpha amplitude for these intervals was similarly determined. The amplitude value for each of the sub-intervals during stimulation was then divided by the mean pre-stimulus amplitude value. The resulting ratio, the amplitude ratio (A.R.), thus provided a measure of relative decrement or increment of alpha amplitude at given temporal points during the time of action of the stimulus. In addition to these measurements, in the first series of experiments, similar amplitude ratios were determined for six consecutive 0.50 sec. intervals following the termination of the stimulus.

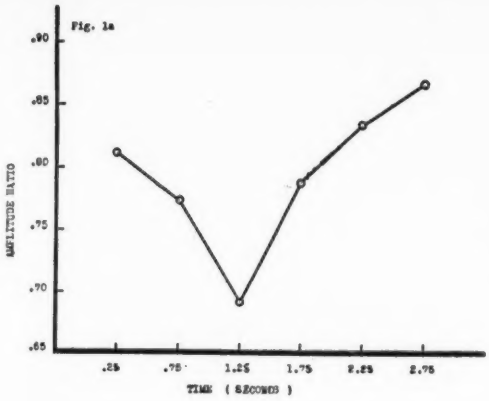
In the second series of experiments, the amplitude ratios were determined for six consecutive 0.333 sec. intervals within the first 2.0 sec. following termination of the stimulus. As before, the ratio was between the total amplitude of the sub-interval and the mean of equal intervals in the 1.0 sec. preceding the delivery of the stimulus.

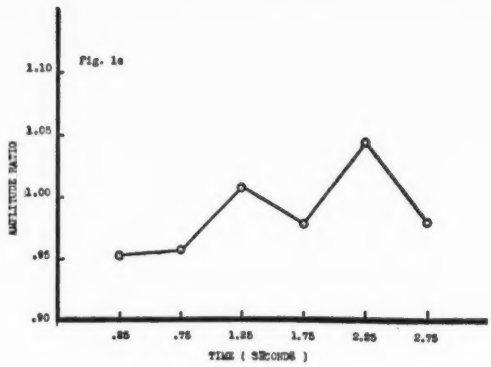
In the first series of experiments four *Ss* were used; in the second, 10 *Ss*.

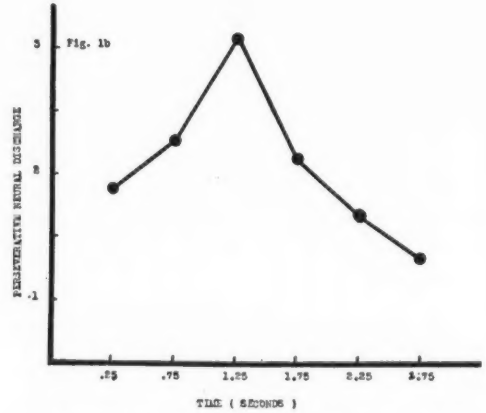
Results

Series One:

The amplitude ratios for each of the 0.5 sec. intervals during the presentation of the 3.0 sec. tone are presented in Table I. The stimuli







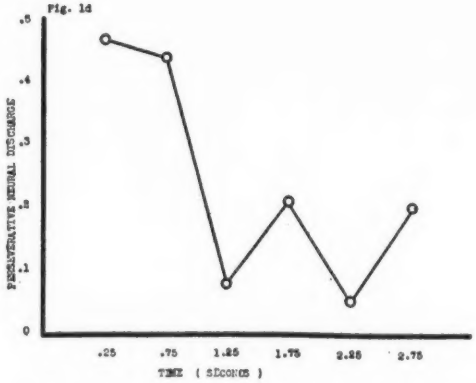


FIG. 1a. Amplitude ratio at each .5 sec. interval during three-sec. auditory stimulus.

FIG. 1b. Perseverative neural discharge (P.N.D. = 10 (1.0—A.R.)) for each .5 sec. interval during three-sec. auditory stimulus.

FIG. 1c. Amplitude ratio for each .5 sec. interval following the three-sec. auditory stimulus.

FIG. 1d. Perseverative neural discharge for each .5 sec. interval following the three-sec. auditory stimulus.

TABLE I

AMPLITUDE RATIOS FOR EACH 0.50 SEC. INTERVAL (I) DURING THE PRESENTATION OF A 3.0 SEC. AUDITORY STIMULUS

Stimuli	I_1	I_2	I_3	I_4	I_5	I_6
1–5	.624	.589	.503	.600	.550	.546
6–10	.799	.879	.962	.990	1.048	1.159
11–15	.877	.765	.553	.717	.739	.687
16–20	.998	.959	.854	.921	1.050	1.082
21–25	.755	.675	.584	.713	.784	.863
Average A. R.	.811	.773	.691	.788	.834	.867

have been grouped in blocks of five in order to show the variability occurring with repeated stimulation.

These data are graphically presented in Fig. 1a. They serve as a possible measure of the central changes occurring concurrently with the stimulus (S) and the afferent neural impulse (s). The 'perseverative neural discharge' value was obtained by subtracting the A.R. value from 1.0, and multiplying this value by 10.0; when plotted graphically (Fig. 1b), this serves as a direct indicator of the concurrent central processes.

The amplitude ratios for similar intervals in an equal period following the termination of the stimulus are presented in Table II.

TABLE II

AMPLITUDE RATIOS FOR EACH 0.50 SEC. INTERVAL (I) FOLLOWING THE PRESENTATION OF A 3.0 SEC. AUDITORY STIMULUS

Stimulus	I_1	I_2	I_3	I_4	I_5	I_6
1–5	.833	.770	.973	1.040	.973	.867
6–10	.903	1.087	.973	.880	.980	1.000
11–15	.817	.793	.997	.837	.977	.870
16–20	1.150	1.077	1.140	1.197	1.340	1.150
21–25	1.063	1.067	.957	.943	.957	1.013
Average A. R.	.953	.956	1.008	.979	1.045	.980

A graphical representation of these data is given in Fig. 1c, and the perseverative trace in Fig. 1d, which may be taken as the form of the perseverative effects of (s), the afferent neural impulse. Due to the fact that there is little variability of A.R. from interval to interval, the A.R. scale (Fig. 1c) and the perseverative neural discharge scale (Fig. 1d) have been amplified.

Series Two:

The amplitude ratios for each 0.333 sec. interval of the two-sec. period following the termination of the 0.20 sec. stimulus are presented in Table III.

Fig. 1b. Perseverative neural discharge (P.N.D. = 10 (1.0—A.R.)) for each .5 sec. interval during three-sec. auditory stimulus.

Fig. 1d. Perseverative neural discharge for each .5 sec. interval following the three-sec. auditory stimulus.

TABLE III

AMPLITUDE RATIOS FOR EACH 0.333 SEC. INTERVAL (I) DURING THE 2.0 SEC. PERIOD FOLLOWING THE 0.2 SEC. STIMULUS

Stimuli	I_1	I_2	I_3	I_4	I_5	I_6
1–5	.737	.619	.611	.804	.811	.825
6–10	.814	.866	.894	.988	.944	1.058
11–15	.764	.817	.851	.980	1.060	1.073
16–20	.818	.880	.865	.974	.944	.966
21–25	.846	.755	.882	.945	.792	.913
Average A. R.	.796	.787	.821	.938	.910	.967

These data are also presented graphically (Fig. 2a). The 10(1.0−A.R.) values serves as an indicator of their applicability to the concept of the perseverative tendency of (s).

DISCUSSION

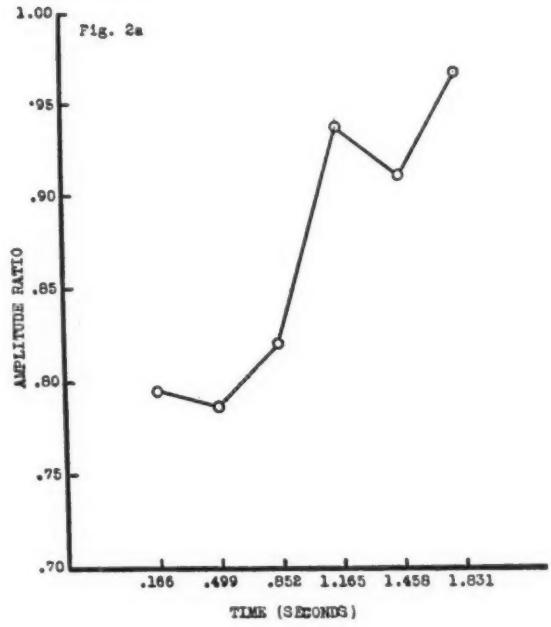
The extent to which these data bear upon the concept of stimulus trace depends upon their general conformity to the predicted time course of the trace.

The formulation bearing upon the concurrent central events obtaining when a receptor is stimulated has been subjected to test by the first part of the Series One experiments. As was seen in Figs. 1a, and 1b, a gradient is apparent, following, in essential form, the postulated characteristics of concurrent central events related to (s).

The second formulation, that which concerns central events following the removal of the stimulus, also finds such a counter-part in the second part of the first experiment, although there is a flattening of the curve which masks the finding unless a large amplification of the A.R. continuum is employed. This flattening may well be due to the long sub-interval measured (0.50 sec.) as well as to factors either not controlled or uncontrollable.[2]

The series based on amplitude change in the alpha rhythm following the 0.20 sec. tone was originally designed to bear on the factor of length of sub-interval. We also felt that duration of stimulus presentation might be important, for the 'meaning' of any given stimulus to the adult human organism might well vary with a long, repeated series of identical stimuli of more than brief duration. The plotted data (Fig. 2a) and their perseverative counterpart (Fig. 2b) follow essentially the 3.0 sec. data following stimulation. There

[2] Examination of the tables shows there to be considerable variation in the A.R.'s from one block of the trials to the next. This variation might be ascribed to 'adaptation' (Bagchi, 1; Travis and Knott, 11, 12), to 'inhibition' (Knott and Henry, 7), or to 'set' (Bakes, 2). Which, if any, of these factors is operating, if not all—and perhaps more—cannot be stated. It is clear that further investigation of this problem is necessary.



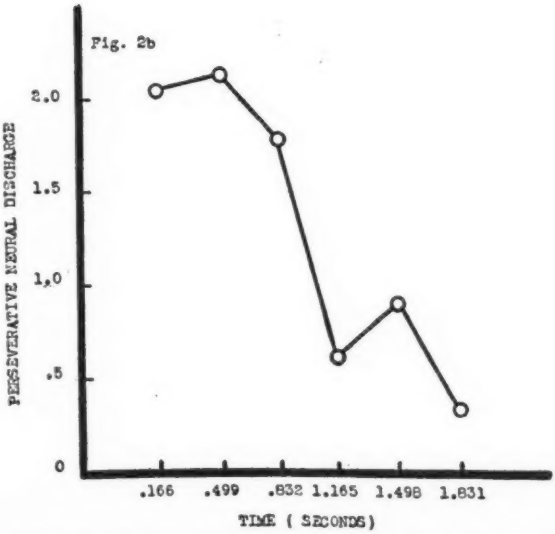


FIG. 2a. Amplitude ratio for each .333 sec. interval following .2 sec. auditory stimulus.
FIG. 2b. Perseverative neural discharge for each .333 sec. interval following .2 sec. auditory stimulus.

is also some degree of similarity between the 0.2 sec. data and the 3.0 sec. data during stimulation.

The crucial test of whether or not a maximum decrement in alpha energy, following a stimulus, may be equated with maximum in-

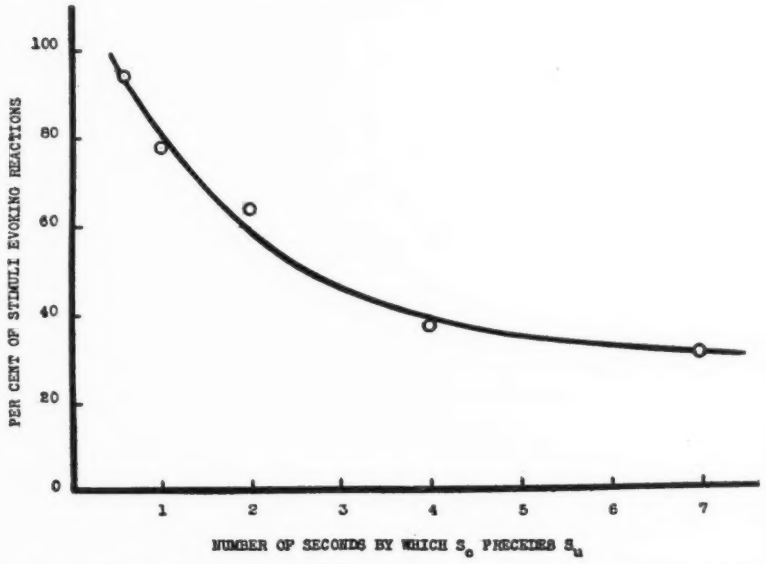


FIG. 3. Strength of conditioned response as a function of time interval between S_c and S_u. (Data from Kappauf and Schlosberg (6); redrawn from Hull (8).)

crement of the central effects of the afferent neural impulse (either concurrent or perseverative) must be found by a correlated study which measures simultaneously the EEG change and the strength of conditioning. If it were to be found that the time interval between onset of stimulation and maximum reduction in alpha for a given stimulus (which is the conditioned stimulus) was the same time interval which provided maximum strength of conditioning, then, and only then, could one conclude that the measures here discussed do serve as an index of cortical processes which are, or are related to, the afferent neural impulse.

In the meantime, we feel that there is a striking parallel between certain conditioning data and the temporal gradient of change in alpha energy following stimulation. Kappauf and Schlosberg (6) found that when they varied the time by which the conditioned stimulus preceded the unconditioned stimulus, the strength of the conditioned response was greater, up to a point, when the time

interval between S_c and S_u was shorter. The conditioned and unconditioned stimuli were over-lapping in that they terminated at the same time. From these data Hull (8, p. 166) has abstracted an ideal curve supporting his concept of stimulus trace.[3] This is pre-

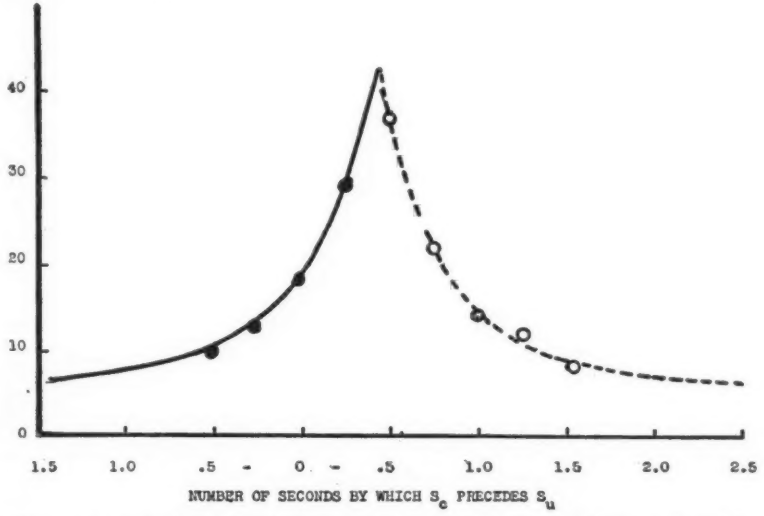


FIG. 4. Strength of conditioned response as a function of time interval between S_c and S_u. (Data from Wolfle (13); redrawn from Hull (8).)

sented in Fig. 3, and a gross resemblance between it and Fig. 1b is at once apparent, although there is a difference in the time to maximum of the two curves. This difference might be assigned to the differences in stimuli used in the two experiments, which would presumably affect the trace.

Wolfle (13) paired an auditory stimulus (a single click) and electric shock to the hand. Dividing her *S*s into equal groups and varying the S_c–S_u interval from group to group, she found that the strength of the conditioned response was greatest when the interval was .5 sec. Longer or shorter intervals produced a decrease in strength of the conditioned response. Hull has also presented an analysis of these data (8, p. 170), which serves to indicate the course of the perseverative trace following termination of the stimulus. This generalized curve is presented in Fig. 4 and the right-hand portion of this curve may be compared with Fig. 2b. It will be ap-

[3] While such formulations are exceptionally useful, we feel that one must be somewhat cautious in assuming too close a correspondence between stimulus and central effects, or between sense-organ process and central effects, for there is some evidence of alteration of temporal patterns within neural pathways lying between the sense organ and the central terminal (Marshall, 9).

parent that there is a remarkable similarity between these two sets of data.

From these comparative data we may conclude, for the time being, that the electroencephalogram may offer one neurophysiological index of such a molar theoretical variable as Hull's concept of stimulus trace.

Summary and Conclusions

This research was planned to provide a quantitative neurophysiological investigation of the behavioral concept of stimulus trace. Changes in alpha amplitude during and following the presentation of auditory stimuli served as the measure.

The general form of the temporal gradient of alpha amplitude following a single stimulus follows that postulated by Hull for perseverative trace and also follows the behavioral data from which the concept of trace have been deduced.

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AN EXPERIMENTAL STUDY OF THE ROLE OF THE EGO IN WORK. I. THE ROLE OF THE EGO IN COÖPERATIVE WORK [1]

BY HELEN BLOCK LEWIS

For many people in our society, responsibility for personal welfare, for success and failure, rests solely upon the individual. The result is a tendency for many individuals to develop a characteristic pattern of activity in which the ego is focal and the objective situation, background. So many aspects of the objective world, on the other hand, by their intrinsic properties demand and absorb the individual's attention, that for a good portion of his life's activities man's ego recedes into the background of behavior. One may think of these two fundamentally different approaches to reality as 'indirect' via ego wishes and needs, and 'direct,' with the objective situation reacted to, manipulated 'for its own sake.' The contrast, for example, between a child constructing a toy house with a glance every alternate minute at his teacher or parent, and a child completely and selflessly involved in the same task, illustrates the basic difference in approach to the world which forms the subject of this paper.

It is necessary first to clarify the sense in which we are using the term 'ego.' 'Ego' needs mean, in this discussion, 'selfish' needs, that is, needs restricted in scope to the enhancement of the self. The goal of a person whose 'ego' needs are focal is the achievement of some pleasure, reward or improved status for his *self*. The person pursues activity with this self-enhancement or self-reward as his *goal*, to the exclusion of all other demands either from the objective environment or from other people. 'Ego' needs mean, then, in our discussion, 'self' or 'egotistical' needs.

This very narrow conception of the ego is introduced here mainly for purposes of clarity. It is essential that a distinction be drawn between ego or selfish needs, and the needs of the person. If the ego and the person are made synonymous, as is often done, then any

[1] The experiment reported in this paper was performed together with the author's student, Miss Muriel Franklin. Miss Franklin participated actively in the planning and preparation of the experiment and in its administration. Without her the experiment could never have been performed.

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need of the human organism becomes ipso facto an ego need. By restricting the use of the term, ego, to situations in which the person functions selfishly or egotistically, we at least achieve a clear terminology for this report.

Hedonistic theories of motivation have characteristically described only one pattern of relationship between the individual, his goal, the task and reward or 'satisfaction': the individual enters upon and completes a task in order to secure reward or 'satisfaction.' The goal of the individual is thus an ego goal—the attainment of reward—and the completion of the task is a means toward the completion of this goal. Satisfaction in this case need not accompany just the completion of the task, since the goal of the individual and the requirements of the objective situation do not necessarily coincide. Completion of the task will bring satisfaction only if task-completion results in some additional, extrinsic reward for which the person has been striving. Task-completion is just as likely to result in an even further widening of the distance between the individual and his goal of pleasure (reward). Familiar examples of the operation of this pattern of motivation in work are not hard to find: one thinks of the child working at his arithmetic examples in order to achieve a gold star. A child strongly motivated toward the achievement of a gold star may derive no pleasure from the solution of arithmetic problems, which may or may not achieve the teacher's approval.

Schematically, the hedonistic conception of the basic motivational pattern in work would appear as follows:

P————Task————→Ego-goal (pleasure; reward)

That is, the person (P) uses the task as a means to attain an ego-goal: pleasure or reward.

There are, however, many occasions in which the relationship of the individual to his goal in work is much more simple and direct than hedonistic theory would imply. The goal of the individual is the completion of the *task*, so that his goal and the requirements of the objective situation coincide. In these cases, satisfaction or the release of activity tension in work coincides with the completion of the task, but was not the aim of task-completion.

Schematically represented, this simpler pattern of relationship would appear as follows:

P————Task————→Task-goal (task-completion)

The only real sense in which the task goal in this scheme is a personal goal is that it is the goal of the person. The *nature* of the goal is to complete the task.

On still other occasions, man enters upon and pursues tasks in order to help others or to help achieve an ideal. In such cases, the person's aims are not restricted by his own self-demands, which are often pushed aside. The person's needs are, on the contrary, so broad that they include the needs of others, or the needs of the ideal. For these occasions, hedonistic theory has no place at all. This theory characteristically deals with such cases only by distorting the real meaning of the term 'self': given a person displaying this selfless behavior, then selflessness must also be a selfish pleasure—this leads us, by circular reasoning, to an absurd conclusion.

Schematically represented, the relationship between the person, task and goal on these occasions is the same as in the second scheme outlined above. The difference is simply in the breadth of the task-goal.

From this discussion it would follow that, in many instances, the person is objectively oriented, pursuing directly the solution of problems posed by his environment without necessarily pursuing hedonistic aims. This is not to assert that man's physical and psychological needs do not motivate him to action; it does assert, however, that man's needs to action are often aroused by the intrinsic properties of the objective situation which he confronts (including the needs of others), and that satisfaction in the completion of a task therefore results without the addition of rewards extraneous to the task itself.

Certain propositions about competitive and coöperative work derive from these general considerations.

1. A minimum requirement for coöperative behavior is not physical togetherness nor joint action, nor even synchronous, complementary behavior, but a diminution of ego-demands so that the requirements of the objective situation and of the other person may function freely. In truly coöperative work, personal needs can function only as they are relevant to the objective situation; the common-objective, in other words, is more important than any personal objective. In this respect, similar behavior should be expected of the coöperating group and the highly 'individualistic' scientist absorbed in his task. Since the self is not focal, another person's activities—the coöperating person's—may be as satisfactory as your own.

2. Competing for individual rewards, i.e., individualistic competition, on the other hand, involves a heightening of ego-demands, so that the ego-objective is more important than any common objective; i.e., the person is at the focus of consciousness. Self-consciousness is at a maximum—the individual is 'on the spot' so that similar behavior may be expected from the member of the competing group and the person driven by inordinate (neurotic) ambition (Horney, 1).

Competing behavior involves seeing the objective situation as relevant to the personal need to win, or for prestige. Only personal activities, therefore, can be satisfactory.

Two predictions follow, further, from these considerations:

1. Satisfaction in work should be obtainable from the coöperating person's activities as well as from one's own. Since the objective situation is focal, rather than the ego, the actual agent in dealing with the objective world need not necessarily be one's self. What the other person does may be as important, as satisfying as one's own activities.

2. When the ego is focal, when the objective situation is seen only as it relates to the dominant ego-needs, then activity will be directed toward *only part* of the objective situation, principally that part which offers satisfaction for ego-demands. The parts of the situation which satisfy the ego will stand out; the parts which wound it, or do not satisfy it may be avoided, or even repressed.

The experiments to be reported here, and in a second report in this series (**2**), were designed to study in detail the nature of work undertaken in one of the two quite different contexts described above. Since Zeigarnik's first experimental demonstration of 'tension-systems' (**4**), there has been a growing amount of evidence indicating that the individual's recall of a series of tasks he has done is a fairly sensitive indicator of attitudes operating in the work situation. We chose therefore to utilize the experimental technique of Zeigarnik as the basis of our study.

Experiment CW

A. Procedure

In order to create a situation in which work to be done would be focal, and the pressure on the individual's ego minimal, a *coöperative work* situation was devised. Each of 14 Brooklyn College students performed 18 heterogeneous tasks jointly with another student who acted as "planted co-worker."[3] Each *S* was obtained by the planted co-worker who asked each person to "come and help her do some work for" the author. The *S* was brought by the planted co-worker to the author's office, where the experiment was actually performed. On entering the office the *S* was told by the planted co-worker that "we have lots of things to do. Let's begin by ————(the first task)."

Half the tasks were casually interrupted by the co-worker who said: "Oh, I'll finish that up," and proceeded to take the task away from the *S* and finish it. The other nine tasks were completed by the *S* alone after the co-worker had quietly withdrawn, saying; "Oh, you finish that." All of the tasks were thus jointly begun by the *S* and co-worker and completed either by the *S* or co-worker alone.[4]

The tasks were performed by the *S* and *CW* in a friendly, informal, *work* atmosphere. No suspicion obtained at any time (except in the case of one *S* who 'wondered a while') that a psychological experiment was being performed. A few *S*s were friends of the planted *CW*, who

[3] Miss Muriel Franklin served as planted co-worker in this and subsequent experiments.

[4] The subject will be referred to as *S*, the co-worker as *CW*. Tasks finished by the subject will be called *S* tasks; those finished by the co-worker, *CW* tasks.

was known to be the author's NYA assistant. The fact that the experiment was performed in the author's office at Brooklyn College enhanced the notion that this was real work in preparation for the experiments of the Psychology Department.

A third person, the observer, was also present in the room during the experimental session. This person was also supposed to be doing NYA work, but entirely unrelated to the work of the *S* and *CW*. The observer actually kept a running record of the time of each task, and of the *S*'s comments. The observer was seated at a desk in a corner of the room quite removed from the experimental table and was quite unnoticed by the *S*s.

The tasks took an average of five min. each to complete. The *CW* interrupted or withdrew to allow the *S* to complete the task after about three or four min. of work. The entire work session took about 1½ hours per *S* and was followed by a ½ hour interview.

Each task was administered as an *S*-completed (completed) task for about half the *S*s, and as a *CW*-completed (interrupted) task for the other half.

At the conclusion of the 18 tasks the planted *CW* took the *S* out of the room (to avoid any cues in recall) and asked: "What tasks did you perform during the last hour or so?" After the recall, a fairly extensive interview took place covering the following questions: attitude toward coöperation, interpretation of 'interruption,' interpretation of the purpose of the work, tasks liked and disliked, etc.

There were, of course, difficulties in the administration of the coöperative work procedure. One *S* (No. 2) felt that the *CW* was 'bossy' when she said, "Oh, you finish that." This same *S* "wondered what he was needed for" while the co-worker was finishing the tasks. Another *S* (No. 3) felt that she was "all armsy and legsy" while the *CW* was finishing. The same *S* also resented the "break in the progress of work" which finishing alone entailed. Another *S* (No. 12) felt it would have been more efficient coöperative work if each had worked alone on all tasks, simply sharing the quantity of work done. He disliked the interruptions, and often told the co-worker to "work on her stuff" without bothering him.

These were inevitable awkwardnesses resulting from the fairly rigid requirements of the experimental design. By and large, our protocols reveal, the *S* did feel that he was participating in a smoothly running, fairly natural coöperative work situation.

B. Results

Table I shows the recall results for each of our 14 *S*s. It will be seen at once that 11 out of our 14 *S*s show a maximum difference of only one task between RI and RC. Of these 11, 8 *S*s show RI/RC ratios of exactly 1.00.

The average ratio is 0.94, which is in striking contrast to Zeigarnik's ratio of 1.90. Zeigarnik found that interrupted tasks are recalled nearly twice as often as completed ones. Experiment *CW* demonstrates that interrupted tasks *completed by a coöperating partner* are not recalled any more often than self-completed tasks. In Ziegarnik's terms, the 'tension-systems' aroused to complete a task is resolved by the activities of someone other than oneself—by the cooperating partner.

If the statistically indefensible procedure of averaging ratios is abandoned, and the ratio calculated between the average number of Completed tasks recalled (Ave. RC) and the average number of Interrupted tasks recalled (Ave. RI), the result is 0.88, which is again in striking contrast to a corrected ratio of 1.61 obtained by Zeigarnik. (See Marrow, 3.)

If completion by a coöperating worker is satisfactory, then the end of our experimental session presumably saw the task-completion

TABLE I

SHOWING RI, RC, RT, RI/RT, RI/RC FOR EACH *S* IN COÖPERATIVE WORK EXPERIMENT

Subj.	RI	RC	RT	RI/RT	RI/RC
1	5	3	8	.63	1.67
2	5	4	9	.56	1.25
3	3	3	6	.50	1.00
4	5	5	10	.50	1.00
5	4	4	8	.50	1.00
6	4	4	8	.50	1.00
7	4	4	8	.50	1.00
8	3	3	6	.50	1.00
9	4	4	8	.50	1.00
10	4	4	8	.50	1.00
11	4	5	9	.44	0.80
12	3	4	7	.43	0.75
13	3	5	8	.38	0.60
14	1	7	8	.13	0.14
Means	3.71	4.21	7.92	.47	(0.94)

RI = number of interrupted tasks recalled
RC = number of completed tasks recalled.
RT = total number of tasks recalled.

$$\text{Ratio } \frac{\text{ave. RI}}{\text{ave. RC}} = 0.88$$

$$\text{Ratio } \frac{\text{ave. RI}}{\text{ave. RT}} = 0.47$$

tension systems of most *S*s altogether resolved. Their recall of interrupted and self-completed tasks should therefore theoretically be a function of chance, and we should have an equal number of ratios above 1.00 and below 1.00 in our 14 *S*s. The expected distribution of ratios should, therefore, be 7 below 1.00 and 7 at 1.00 or above. The obtained distribution of ratios is 4 below 1.00 and 10 at 1.00 or above. Applying the chi-square test to this difference between expected and obtained distributions we find a P-value of 0.10. Our hypothesis is thus statistically confirmed. The distribution of ratios obtained in the *CW* experiment does not differ significantly from the distribution one would expect by chance.

1. *Protocols*

The protocols of the *S*s make perfectly clear the reason for these results. In the first place, all eight *S*s whose ratio was 1.00 did not consider *unfinished* the tasks they had been prevented from finishing personally. They insisted, on the contrary, that they had finished all the tasks. As *S* No. 8 put it, "It didn't make any difference who finished the task. The task was just as complete in all cases, because I had the general idea, and if you finished it off it didn't matter." Another protocol makes the same point: "I finished them all. Of course I did." Or still another *S* said: "Tasks bind people together. Even if they didn't know each other before, they would be friendlier

afterward because they had exchanged ideas and worked together. [This *S* did not know the *CW* before they began the experiment.] If they worked together in space but not toward a common goal this might not happen."

Even more interesting from the protocols was the clear indication that the *S* could not always remember which tasks had been finished by whom. Unfortunately, sufficiently systematic record was not made of the *S*s' memory on this point for statistical analysis to be possible. But the main point of these protocols was that all the tasks were considered *finished.*

The coöperative situation, in other words, engendered for the majority of *S*s the kind of work-attitude in which the actual agency of task-completion became unimportant, as long as the tasks were finished. Tension systems aroused by the tasks could be and were, for these 11 *S*s, resolved by the activity of another person, the co-operating worker.

Three of our *S*s showed a difference of more than one task in the recall of completed and interrupted tasks. For two of them, the difference favors the completed tasks (Nos. 13 and 14); for the remaining *S* (No. 1) the difference favors the interrupted tasks. The protocols of these *S*s make plain the reason for their recall. *S* No. 1 was the *S* who vacillated between suspicion that "it was an experiment" and the belief, especially when the planted co-worker was helping her, that maybe it "wasn't an experiment." She said: "In the situation when I felt you weren't experimenting, whether I finished or you finished made no difference. It was complete . . . If you were experimenting on me, I felt I should have a chance to finish."

S No. 13, who recalled three interrupted and five self-completed tasks, shows quite a different process. This *S* felt that there were "really few things we did together." "I was only doing what you told me to. . . . I was more aware of those tasks which I completed." It is significant that this *S* spontaneously mentioned his feeling that working together with someone was 'reassuring.' Apparently, this *S* retained a good deal of ego-orientation despite the apparently coöperative situation.

S No. 14 illustrates this process in an extreme and clarifying fashion. The recall here consisted of one interrupted and seven self-completed tasks. This *S* felt that "it was my fault rather than yours that I shirked my duty and wasn't coöperative enough. . . . When you finished I felt it was unfair on my part because I felt I was shirking (?) what I should have done." Despite the nature of the work situation, then, this *S* remained under pressure to maintain her ego-status. Questions of fault, of the relative merit of her contribution,

were dominant in her attitude toward the work. As a consequence, tasks which the *S* was not permitted to finish became failures, while tasks which she herself finished gave her considerable satisfaction.

S No. 14 was, in other words, ego-oriented despite the apparently coöperative situation in which she was working. Her recall, both of interrupted (*CW*) and self-completed (*S*) tasks is unusual. Interrupted tasks, which she seemed to regard as her 'failures' were absent in recall; self-completed tasks, which apparently were 'successes,' were present in great proportion. The reasons for this pattern of recall are not yet clear, although many interesting speculations could be raised. All that can be said, however, so far, is that this *S*'s ego-orientation resulted in an extreme recall ratio.

2. *Task-Analysis*

We turn next to an analysis of the relationship between recall of tasks in coöperative work and the nature of the tasks themselves. The 18 tasks used in this experiment were:

1. *Winding* thread on a spool: *S* holds the spool and *CW* winds, or vice versa.
2. Solving *Anagrams:* a set of 20 disarranged letters, all names of fruits and vegetables. *S* and *CW* discuss possible solutions, and make them jointly. Some division of labor takes place. It was necessary here for the *CW* to hold back solutions, so that the solutions would appear to be joint, and equally contributed to by *S* and *CW*.
3. *Clay* modeling: *S* and *CW* work jointly to copy a model clay house.
4. *Limerick:* This was made into a routine task of copying, i.e., tracing limericks from a printed page to another page. *S* and *CW share just the amount of work. S copies one; CW another* limerick, etc.[5]
5. *Cutting and pasting* an article from a newspaper into a scrapbook. *S* and *CW* assisted each other, held paper, wiped paste, etc., for each other.
6. *Adding* a set of three-column numbers. *S* added one column aloud while the *CW* checked; then the *CW* added the other column, while *S* checked.
7. *Copying map* of distribution of Indian cultures in North America. *S* dictates and *CW* copies, or vice versa.
8. *Packing* books and magazines into a box so that the lid closes. *S* and *CW* assist each other—hand each other books, pile, stack, etc.
9. *Alphabetizing* index cards. *S* and *CW* simply share the quantity of work.
10. *Stick problem:* To make 4 boxes of equal size out of 9 squares, with 5 sticks remaining. *S* and *CW* discuss each other's hypotheses.
11. *College plan drawing:* To draw a plan of Brooklyn College, showing principal buildings, walks, etc. *S* and *CW* discuss alternative methods of showing plan.
12. *Jigsaw puzzle:* An 18 piece jig-saw puzzle to be put together with a prepared model visible. *S* and *CW* assist each other to find parts.
13. *Circling vowels:* To draw a circle around each of the vowels in a mimeographed paragraph. *S* and *CW* start from oposite ends of the paragraph, i.e., simply share the quantity of work.
14. *Stapling:* Sorting and stapling together a mimeographed reading list. *CW* sorts and *S* staples, or vice versa.
15. *Rearranged sentences:* 10 sets of disarranged words each to be made into a meaningful sentence. *S* and *CW* discuss alternative possibilities. The procedure is much the same as in Anagrams.

[5] In the other experiments this task, using one of the same limericks, was completely changed in character. In the subsequent experiments the task became to complete the last four lines of a limerick beginning: "There was a young lady from Kent."

16. *Braiding* a length of colored wool. *S* holds and *CW* braids, or vice versa.
17. *Copying list:* A list of psychologists names to be copied from the APA yearbook. *S* dictates and *CW* copies, or vice versa.
18. *Writing a letter:* Copying the text of a letter written by the author informing the Psychology Division of a forthcoming seminar. *S* reads and *CW* writes, or vice versa.

These tasks were chosen to resemble, as far as possible, the tasks used by Zeigarnik. It will be seen, of course, that the actual nature of coöperative work is different for different tasks. In some, for example, there is division and specialization of labor—*S* dictates, *CW* copies, or *S* holds and *CW* braids. So in tasks 1, 4,[6] 5, 7, 8, 14, 16, 17, 18, *S* and *CW* are each engaged in different operations which fit together to make the whole task. We shall call these tasks Specialization of Labor tasks (*SL*). In other tasks, *S* and *CW* simply split a given amount of work in half and each does his stint. Here there is division without specialization of labor. Both are engaged in similar or identical operations which, added together, make the whole task. This is true of tasks 2, 9, 13 and to a certain extent, 6. We shall call these tasks Equivalent Halves tasks (*EH*). In still other tasks, *S* and *CW* coöperate on a still different level. Here *S* and *CW* each propose a solution, criticize each other and come to an agreement. This is true for tasks 3, 4, 10, 11, 12 and 15. We shall call these tasks Exchange of Ideas tasks (*EI*).

That this difference in type of coöperation is experienced by the *S* is apparent from the protocols. For example, *S* No. 1 says: "Some [tasks] we did together. I couldn't say I finished them alone. Some we were working on and you took away and finished alone, i.e., some we divided up and you finished your part and I finished mine."

The tasks varied also in their internal structure. That is to say, some tasks had clearly defined, objectively unambiguous terminations, for example, the jig-saw and the stick problems. Not only was the termination of these tasks clear, but the quality of *completion* did not permit of variation. The puzzles were either solved or not solved. (This is not true, of course, of the quality of performance leading to completion but it is true of the completion itself.) The termination of these tasks was inherent, in other words, in the structure of the task itself.

The completion of other tasks depended more upon subjective standards of judgments. For example, the clay house could be finished, but every detail could stand improvement.

For still other tasks, the termination point was an arbitrary matter, set entirely by the instructions of the experimenter. Thus the termination point of vowel circling, stapling papers, is clear, but

[6] See footnote 5.

arbitrary. There might have been 500 vowels instead of 200, 30 sets of papers, instead of 15. Another way of describing the difference in internal structure of the tasks is to say that some are repetitive, consisting of loosely related parts, while others are more highly organized, consisting of more integrated parts. So the jig-saw puzzle and the stick problem are different in structure from the cancellation of letters task or the addition task. Roughly speaking, tasks 1, 4,[7] 5, 6, 7, 8, 9, 13, 14, 16, 17, 18 can be classed as repetitive or routine (*R*), as contrasted with tasks 2, 3, 10, 11, 12, 15 which are more highly integrated (*NR*) tasks.

TABLE II

Task Analysis

Showing Recall Ratios for Each Task in *CW* Experiment

No.	Task	Type	C	RC/C	I	RI/I	RT/T
1	Winding	R–S.L.	6	.33	8	0.00	.14
2	Anagrams	NR–EI	8	.25	6	.83	.50
3	Clay House	NR–EI	8	.38	6	.50	.43
4	Limerick	R–EH	7	.42	7	.42	.43
5	Cutting and Pasting	R–S.L.	7	.29	7	.14	.21
6	Adding	R–EH	9	.54	5	0.00	.36
7	Copying map	R–S.L.	5	0.00	9	.40	.14
8	Packing Box	R–S.L.	9	.54	5	.20	.43
9	Alphabetizing	R–EH	8	.50	6	.33	.43
10	Stick Problem	NR–EI	4	.75	10	.40	.50
11	Drawing College Map	NR–EI	8	.38	6	.50	.43
12	Jig-saw Puzzle	NR–EI	6	.67	8	.50	.57
13	Circling Vowels	R–EH	7	.71	7	.56	.64
14	Stapling	R–S.L.	12	.58	2	0.00	.50
15	Rearranged Sentences	NR–EI	4	.75	10	.80	.78
16	Braiding	R–S.L.	7	.67	7	.50	.64
17	List-Copying	R–S.L.	8	.13	6	.17	.14
18	Letter-Copying	R–S.L.	4	.50	10	.60	.57

C = Number times presented as a completed task, i.e., as a self-completed task.

$$RC/C = \frac{\text{Number completed tasks recalled}}{\text{Number completed tasks presented}}.$$

I = Number times presented as an interrupted, i.e., co-worker-completed, task.

$$RI/I = \frac{\text{Number interrupted tasks recalled}}{\text{Number interrupted tasks presented}}.$$

$$RT/T = \frac{\text{Total number tasks recalled}}{\text{Total number tasks presented}}, \text{ i.e., recall value of task.}$$

It is apparent at once that the classification into routine and non-routine tasks exactly parallels our classification according to the nature of the coöperative relationship. Routine tasks are also the ones in which coöperation took the form either of division and specialization of labor or equivalent halves of work. This overlapping

[7] The *Limerick* was a routine copying task in the CW experiment, but became a non-routine 'creative' task in the other experiments.

between nature of coöperation and type of task is not, of course, accidental, but neither is it an inherently necessary overlapping. It might have been possible to include in our series tasks which were not routine, and still were done jointly by division of labor. Or the same tasks used in this series might have been performed coöperatively in different ways—for example, the jig-saw puzzle or the clay house could have been divided in half (each person completes his share).

The type of coöperation employed for routine and non-routine tasks in this experiment was simply the easiest and most 'natural' form of coöperation for each. It is obvious that routine laboratory tasks will lend themselves better to division and specialization of labor than non-routine tasks.

This overlapping, of course, is unfortunate for purposes of analysis now, because it is apparent from inspection of Table II that routine, division-of-labor tasks show a slightly different pattern of recall ratio than non-routine, exchange-of-ideas tasks. The routine, division-of-labor tasks are more likely to be recalled when self-completed; the exchange-of-ideas tasks, when interrupted. Our general result in Table I seems then not to be independent of the nature of the task, but an overall result, depending upon the fact that we had in our series two kinds of tasks—routine, division-of-labor tasks better recalled when self-completed, and non-routine, exchange-of-ideas tasks better recalled when interrupted. The distribution of recall ratios in these two categories is apparent in Table III. (There

TABLE III

SHOWING RECALL RATIOS ACCORDING TO NATURE OF TASK AND NATURE OF COÖPERATIVE RELATIONSHIP

Task	N	Ratios		
		RC>RI	RI>RC	RI=RC
SL } EH } ; R	12	8	3	1
EI; N-R	6	2	4	0

SL = specialization of labor tasks.
EH = equalization of halves tasks.
R = routine tasks.
EI = exchange of ideas tasks.
N-R = Non-routine tasks.

is no difference in *size* of ratio between the two kinds of tasks, just a difference in direction.)

If one considers for a moment the processes which lie behind the differences in the recall ratios of the two types of tasks, an explana-

tion is readily apparent. Both the nature of the non-routine tasks and the nature of the coöperative relationship in them were such that these tasks afforded an opportunity for greater personal variability in their performance and consequently greater difference in method of work between *S* and *CW*. The number of details one wishes to include in a clay house, the hypotheses one has to offer in solving the stick problem are all much more variable than the steps it is possible to take in order to wind thread, or add numbers or copy a letter (or dictate it). As a consequence a satisfactory completion of such a task by a coöperating worker, while possible, is much more difficult to achieve. There will be some residual ideas (perfectly good ones) which the other member of a coöperating team will find omitted from the final solution of such non-routine, exchange of ideas tasks, even when the agreement between the two workers is quite thorough. As a consequence, some unresolved tensions remained for our coöperating *S*s in these non-routine tasks, and the recall ratios of these tasks slightly favors the interrupted (*CW* completed) condition.

For routine tasks, it is easier to find a quite satisfactory joint solution and one would have expected the ratios for these tasks to be exactly 1.00, instead of favoring the self-completed condition. The probable explanation of this finding lies, I believe, in certain accidental features of the coöperative relationship established in this particular experiment. The planted *CW*, the one who was being helped, was of necessity something of a leader in the coöperative situation, although she did her best to minimize her own role as 'director' of work. But a certain dependency of the *S*s on the planted co-worker was absolutely unavoidable. The *CW* knew what work was to be done, introduced each task, was the principal working with an 'assistant.' Some inequality of work burden (responsibility) resulted. The slightly greater recall of self-completed routine tasks is probably a reflection of this inequality. Self-completed tasks belonged a little more to the *S* himself (actually, of course, he worked on them a little longer); interrupted tasks belonged a little more to the *CW*. Had there been no original inequality of attitude, even this difference in belongingness might not have arisen. The completion of interrupted tasks by the *CW* was satisfactory, therefore these tasks were less likely to be recalled. Self-completed tasks belonged to the *S* a little more and so were more likely to be recalled.

This, is then, our guessed explanation for the obtained difference in recall ratios of individual tasks. In exchange-of-ideas, non-routine tasks, solution by the *CW* was not 100 percent satisfactory; these tasks are therefore recalled slightly more often in the interrupted condition. Routine, division-of-labor tasks are satisfactorily com-

pleted by the partner. Since, however, the partner is something of the 'boss' in the work, the *S*s' own tasks are slightly better recalled by him than the partner's tasks.

It follows from this analysis that our obtained ratios of 1.00 in this coöperative work experiment depend upon the following basic conditions:

1. *The existence of task-orientation*, so that task solution by another person can be personally satisfactory.

(*a*) Coöperative work is only one condition under which such task orientation occurs.

2. *The use of tasks capable of joint solution.*—It is interesting that these 18 tasks chosen by us to meet an altogether unrelated requirement (resemblance to Zeigarnik's tasks) should all have been capable of joint performance. Even such highly 'individual' tasks as puzzle-solving lent themselves to genuinely joint performance.

(*a*) The chief minimum requirements for satisfactory joint solution are: (1) that the task shall have a clear-cut, objectively determined termination; (2) that there shall be equality between the coöperating workers.

(*b*) These requirements can be satisfied with both routine and non-routine tasks. They can also be satisfied in different kinds of coöperative relationship, whether in division of labor or exchange of ideas.

An important additional set of experiments should be performed on these problems. A systematic variation of type of task, type of coöperative relationship, and degree of equality in coöperation ought to be extremely illuminating.

Certain further additional experiments are also required before the significance of the *CW* Experiment can be fully estimated: (1) a 'control' experiment simply repeating Zeigarnik's experiment in our laboratory; (2) an experiment to test the possibility that no 'responsibility' for work (tension-system to complete a task) arises at all in coöperative work; and (3) an experiment to study the consequences of task completion in a non-coöperative situation. These experiments have been performed and will be reported in the next issue of this *Journal* (2).

Provisionally, then, the results of the *CW* Experiment lead to the following conclusions:

1. Under certain conditions, in this experiment, coöperative working conditions, task completion by another person than oneself can be 'satisfactory.' This is indicated by an equal recall of self-completed and interrupted (partner-completed) tasks in a group of 14 *S*s, and by introspective reports.

2. These results offer evidence that motivation in work need not necessarily be egotistical (hedonistic theory), and that, on the contrary, the person is frequently motivated directly by the demands of the objective situation, including the requirements of another person.

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ASSOCIATIVE INHIBITION IN THE LEARNING OF SUCCESSIVE PAIRED-ASSOCIATE LISTS

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Negative transfer, associative interference, proactive inhibition, and associative inhibition have all been variously applied as titles to the phenomenon of a retardation in the acquisition of an activity as a result of having engaged in prior activity. The term associative inhibition has been applied most specifically to the phenomenon when it appears in paired-associates learning, and still more specifically when the task requires the subject to learn a new response to an old stimulus. Having learned to give response B to stimulus A, a subject will usually show some inhibitory effects in learning response C to stimulus A. The variables which are known to influence the amount of inhibition have been reviewed by McGeoch (3).

Most experimentalists who have dealt with these inhibitory factors have limited their studies to the observation of the inhibition resulting from the learning of a single prior list of words. No experiments have been directed specifically at determining the course of the inhibition in the learning of several successive such lists. Twining (5), in a study on retroactive inhibition as a function of the number of interpolated lists, required *S*s to learn five lists of nonsense syllables for ten presentations each. Twining gives the total correct anticipations occurring on these lists as follows: 32.6, 31.6, 30.0, 29.9, and 28.2 for lists one through five respectively. A slight decrease in the total words acquired on each successive list is noted, and, while these differences are small, the trend is suggestive. Twining, however, gives no data on associative inhibition early in the learning of each list, the locus at which it is most likely to occur.

Bugelski (1) required *S*s to learn four consecutive lists of nonsense syllables. The measure of learning was the number of slide exposures required to learn each list. Two different conditions were employed: (1) lists which were made up of unrelated syllables, and (2) lists in which the same stimuli but different responses were used for each list. Under the first condition there was a definite decrease

[1] This paper is based on auxiliary data gathered in performing a series of major experiments which were submitted as a dissertation to the Graduate College of the State University of Iowa in partial fulfilment of the requirements for the degree of Doctor of Philosophy. The major experiments will be published elsewhere. The writer wishes to express his appreciation to Professor Kenneth W. Spence who directed the research.

in the number of exposures required to learn each successive list thus indicating a positive transfer effect. In the second condition there was little difference in the number of exposures required to learn each list. This would suggest that the positive transfer (indicated by the first condition) may have been counteracted by negative influences. But here again no data are available on the early stages of learning each list.

As a consequence of performing a series of experiments whose major subjects were retroactive and proactive inhibition at recall (**6**), auxiliary data are available on the course of associative inhibition in the learning of successive lists of paired adjectives. In view of the dearth of such data it is felt justified to present them separately.

Procedure

The experiment, from which the data to be presented are taken, required (in part) that the *S*s learn 0, 2, 4, and 6 successive lists of paired adjectives for four trials each prior to the learning of another list which was presented until six correct anticipations were given. These four conditions are set up in Table I.

TABLE I*

Outline of Conditions Used in Determining the Associative Inhibition in the Learning of Successive Lists of Paired Associates

Condition	Successive Prior Lists	Common Lists
I	none	A-B
II	A-D, A-C,	A-B
III	A-F, A-E, A-D, A-C,	A-B
IV	A-H, A-G, A-F, A-E, A-D, A-C,	A-B

* An outline for the entire experiment would include a recall of list A-B after 25 min. The recall of this list was the major purpose for which the experiment was planned.

These four conditions were given to 24 undergraduate *S*s in completely counterbalanced order. On all conditions the common list (A-B) was presented until the *S* had correctly anticipated six responses. On condition I, the learning of the A-B list was preceded by no prior lists; on condition II it was preceded by two lists; on condition III, by four lists; and on condition IV, the common list was preceded by the learning of six lists. All of these prior lists were presented for four trials.

Each list consisted of 10 pairs of two-syllable adjectives chosen according to the usual rules (**4**). Every list for any specific condition had the same stimulus words, these words being designated by A in Table I, but different response words as indicated by B, C, D, etc. Thus, under condition IV, the *S*s might learn seven responses to the same stimulus.

Four sets of seven lists each were constructed. Set No. 1 was used on the first experimental day; set No. 2 on the second, etc., throughout the four sessions. In addition, the six prior lists were counterbalanced within themselves. By counterbalancing the prior lists in pairs, complete equity was attained in each block of six *S*s. The order of the pairs of lists were reversed for the second group of 12 *S*s. Thus, the prior lists that were the odd lists for the first group of 12 *S*s became the even lists for the second group of 12 *S*s.

The lists were presented on a modified Hull drum at the usual four-sec. rate. Forty-five sec. elapsed between the learning of each pair of lists. This time was necessary in order to make the mechanical changes in shifting from list to list.

The instructions given to the *S*s were those which usually precede paired-associates learning. In addition a special paragraph of instructions was read to the *S*s in an effort to maintain a rela-

tively constant level of motivation in going from list to list. Since the prior lists were presented for four trials, the *S*s had nothing to force them to learn the words during these trials.

Two practice days preceded the four experimental days. During these two practice days the *S*s had considerable practice in shifting rapidly from list to list. To balance practice effects during the experimental sessions all *S*s learned the same number of lists each session. These lists were learned after the recall and relearning of list A-B (which was the major purpose of the experiment). Thus, on condition I, six lists were presented for four trials each after the relearning of list A-B; on condition II, four lists were learned, etc. The data presented below are limited to the portion of the experiment shown in Table I.

RESULTS

According to the experimental outline of Table I, the data can be grouped so that for the first two lists in the series of six there are three observations for each *S* for a total of 72. For lists 3 and 4 there are two observations for each *S*, or a total of 48, and for the 5th and 6th lists there is a single observation for each *S*. Hence, the mean numbers of correct responses for the series of six lists are based on different numbers of observations.[2] These means are shown in Table II for each of the three anticipation trials of each list.

TABLE II

ASSOCIATIVE INHIBITION IN THE LEARNING OF SIX CONSECUTIVE LISTS FOR FOUR TRIALS EACH

The mean correct anticipations of lists 1 and 2 represent 72 observations; lists 3 and 4 represent 48 observations, and lists 5 and 6, 24 observations. The lower section of the table indicates the percent of inhibition.

Lists	Trials					
	2		3		4	
	M	σ_M	M	σ_M	M	σ_M
1	1.95	.25	3.36	.28	4.57	.36
2	1.14	.14	2.51	.25	3.54	.31
3	1.27	.19	2.60	.30	3.88	.36
4	1.52	.18	3.15	.29	4.73	.41
5	1.33	.23	3.00	.39	4.75	.38
6	1.33	.24	2.96	.29	4.63	.36
	Percent of Inhibition					
2	42		25		23	
3	35		23		15	
4	22		6		−4	
5	32		11		−4	
6	32		12		−1	

The percent of inhibition is based on the degree of learning shown on the first list in the series. In all measures of inhibition this first list serves as the control.

[2] The learning of the 7th (A-B) list is not included since a few *S*s learned this list to the criterion in less than four trials.

In Table II it is to be noted that on the first anticipation trial (trial 2) the greatest amount of inhibition occurs on lists 2, 3, 5, and 6, with a decrease on the fourth list that is consistent on all three trials.[3] It will be noticed further that the inhibition occurring during

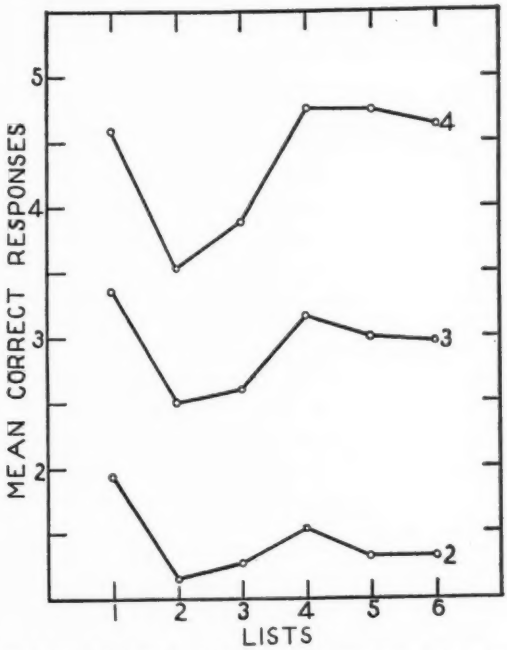

FIG. 1. Associative inhibition in the consecutive learning of six paired-associates lists, showing the mean correct responses on each of the three anticipation trials of these six lists. List one is the control list (preceded by no learning) against which the learning on the other lists should be compared.

the learning of lists 5 and 6 dissipates more rapidly than it does on lists 2 and 3. By the end of the fourth trial there is indication of positive transfer on lists 4, 5, and 6. These facts of Table II are shown graphically in Fig. 1.

[3] This decrease, however, is not statistically reliable on the first trial when compared with list 2. The following measures of reliability will give an indication of the degree of confidence that can be placed in the means of Table II. On the second trial the differences and t ratios between list 1 and lists 2 through 6 are as follows: $.81 \pm .22$ ($t = 3.68$); $.68 \pm .26$ ($t = 2.62$); $.43 \pm .28$ ($t = 1.54$); $.62 \pm .24$ ($t = 2.58$); $.62 \pm .26$ ($t = 2.38$). A t-value of 2.807 indicates a difference at the 1 percent level of confidence while a t-ratio of 2.069 signifies a difference at the 5 percent level of confidence. While all differences are not great enough to admit statistical reliability the rather consistent trends of the curves through the three anticipation trials may argue for the reliability of the smaller differences. Evidence in Fig. 2 would substantiate such an argument.

The indication that positive transfer assumes the dominant role over associative inhibition as learning continues is substantiated further by considering the learning of the common list. This list followed the learning of 0, 2, 4, or 6 lists and was presented until the *S* had correctly anticipated six responses. Fig. 2 gives rather con-

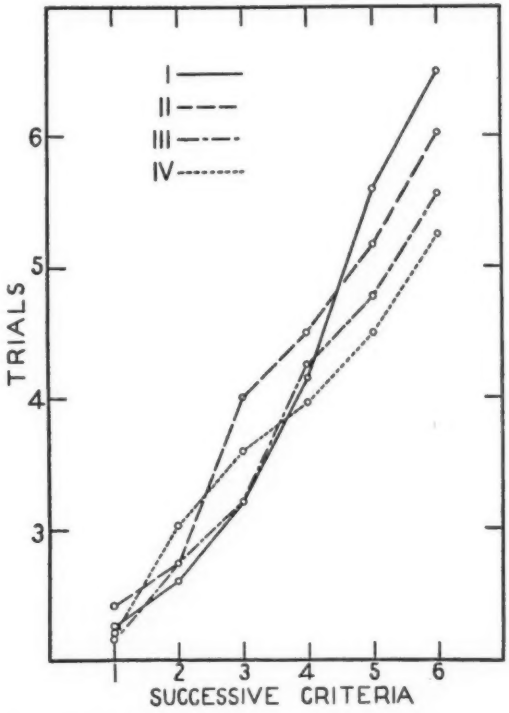

FIG. 2. Learning curves of paired-adjective lists when preceded by 0, 2, 4, and 6 prior lists. The curves represent the mean trials required to reach successive criteria of one correct response, two correct responses, . . . six correct responses.

vincing evidence that the amount of positive transfer which eventually occurred in learning is directly related to the number of prior lists learned.

Fig. 2 shows the mean number of trials required to reach the successive criteria of one through six correct responses. Condition I had zero lists preceding the learning of the common list, and the curve for this condition is to be used for comparative purposes with the others. Condition II had two lists preceding the learning of this common list. On this condition the inhibition persists longer

than in any other condition, but even so, by the time the criterion of six correct responses is reached, positive transfer is indicated by comparing it with the curve for condition I. On condition III (four prior lists) there is little evidence of associative inhibition, and the positive transfer occurs earlier and has reached greater proportions at the end of learning when compared with condition II. Further accentuation of this tendency is indicated by condition IV (six prior lists). There can be little doubt that, although certain factors may delay the appearance of positive transfer as the number of lists increase, it will eventually appear in an amount that is directly related to the number of prior lists learned.

Overt inter-list intrusions appearing during the learning of these lists are rare. The frequency of intrusions on the various lists are shown in Table III. The great majority of these intrusions which

TABLE III

TOTAL FREQUENCY OF INTRUSIONS FOR EACH LIST ON EACH CONDITION

Condition	Lists				
	2	3	4	5	6
II	4				
III	3	7	2		
IV	4	8	1	4	5

occur in the learning of any particular list are words which appeared in the immediately preceding list. Lists two or more steps removed seldom produced any overt competition. Although the frequency of these intrusions for the lists is small, there seemed to be little relation between the serial position of the list and the trial on which the intrusion occurred. That is, the relative frequency of intrusions appearing on the fourth trial of the second list seemed to be no different from the frequency on the same trial of the sixth list. It should be emphasized, however, that the figures in Table III are gross totals for all three trials for all *S*s and not mean intrusions per *S*. This emphasizes the small number of intrusions which actually occurred. The third list in the series produced the greatest number of intrusions (about one for every three *S*s), yet, list 2 showed the greatest amount of inhibition. On the other hand, list 4 exhibited the fewest number of intrusions and it likewise showed the smallest amount of inhibition. The frequency of these inter-list intrusions is so small that little in the way of conclusive statements can be made. Added similarity in the response words would very likely accentuate intrusion trends and bring out differences which cannot be discerned safely in the present experiment.

Discussion

As indicated in the introductory paragraph the data presented here were auxiliary in nature. This portion of the experiment was predicated on no hypothesis. What evidence there was available would have seemed to indicate that associative inhibition would increase as the number of lists increased. The results that have been shown in the preceding section indicate that the problem is far more complicated than anticipated.

One of the most perplexing facts is the decrease of inhibition on the fourth list in the series followed by an increase on the fifth and sixth lists.[4] As mentioned before, there is no statistical assurance that the decrease in associative inhibition on the fourth list is reliable. However, three supporting bits of evidence lead one to believe that this decrease must be reckoned with in any attempted interpretation. (1) In Fig. 2, there is little inhibition evidenced on the learning of the common list of condition III. The learning of this list after four prior lists is commensurate with the learning of the fourth list in the series of prior lists. (2) Very few intrusions appear in the learning of the fourth list in the series as compared with the other lists. As indicated before, the overall frequency of intrusions is too small to justify a great deal of confidence, but in this instance the small frequency does fit into the pattern, indicating that the reduction of inhibition on the fourth list may be real. (3) The decrease in inhibition on the fourth list is consistent throughout all three anticipation trials. One is inclined to believe that the reduction in associative inhibition on the fourth list is not a chance occurrence and that this decrease is replaced by an increase on the fifth, sixth, and seventh lists in the initial stages of learning. Fig. 2 is substantial eivdence that the inhibition appearing early in learning these later lists is quickly superceded by positive transfer.

There is no ready explanation to the data that have been presented. In general, two opposed tendencies may be assumed safely: (1) the deleterious effects of learning a new response to an old stimulus, and (2) the positive effects of practice. Just how practice effects result in increased speed of learning is usually assumed as the 'given' factor in theories of transfer. The best was of *describing* it seems to be as McGeoch (3) did when he said that the subject is "learning how to learn." This may include many things. It may be that the *S* learns how to form associations more rapidly, which is merely restating the fact. It may mean that the *S* becomes less distracted by non-essential factors in the environment. Such a statement would

[4] The mean number of correct anticipations on the second trial of the seventh (common) list was almost identical with that of the fifth and sixth lists.

have some backing from conditioned response studies. It might also mean, in the present experiment, that the *S* learns how to dissociate with greater ease. That is, the *S* learns how to dissociate each new response from the old response and to do it with greater speed the greater the number of responses that have been dissociated or differentiated.

Gibson (2) has used the concept of *stimulus differentiation* to explain the faster learning where the same stimuli are paired with different responses in two or more lists. Increased stimulus differentiation would be indicated by a reduction in intra-list errors. These errors were relatively infrequent in the present experiment, although there is some evidence to support the Gibson hypothesis. In considering the frequency of these intra-list errors during the learning, the common list has been used since the positive transfer was unmistakable, as indicated in Fig. 2. The mean total of errors per trial for this list on condition I (no prior lists) was 14; for condition II (two prior lists) the number was 9; for condition III (4 prior lists), 8, and for condition IV (6 prior lists) the mean total of errors per trial was 7. This evidence, while meager, does suggest that the amount of positive transfer is inversely related to the number of intra-list errors.

The difference between these practice effects (positive) and the negative effects inherent in the A-B, A-C relationship is the measured associative inhibition. The balance between these two factors becomes increasingly complex when the effect of degree of learning is considered. It is known that the degree of learning of a prior list influences the measured associative inhibition on the subsequent list (3). In learning a series of lists, then, the associative inhibition decreases the degree of learning of a list which in turn should influence the associative inhibition of the following list. These factors appear to be so inextricably related that factoring them out on the basis of a single experiment seems unlikely.

Summary

Twenty-four *S*s learned 0, 2, 4, and 6 lists of paired-adjectives presented for four trials each, prior to the learning of another list until six responses were correctly anticipated. The major data presented were concerned with associative inhibition in learning the six consecutive lists. The results show:

1. In the series of six lists, the greatest degree of associative inhibition obtains in the learning of the second list, both in initial amounts and greatest perseveration.

2. There is a decrease in inhibition in the learning of the fourth list followed by an increase on the fifth and sixth lists, but only on

the first anticipation trial. The data show conclusively that the amount of positive transfer which eventually accrues in the successive lists is directly related to the number of prior lists learned.

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'ALPHA' CONDITIONING IN THE EYELID

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Introduction

The purpose of the present study was to investigate the nature and extent of 'alpha' conditioning of the eyelid when the effects of reflex adaptation were taken into account. 'Alpha' conditioning as defined by Hull (**9**, p. 431) is ". . . sensitizations or augmentations of the original unconditioned reaction of the conditioned stimulus . . ." resulting from paired presentation of the conditioned with an unconditioned stimulus.

The status of 'alpha' conditioning in the eylid response system has recently been reviewed by Weber and Wendt (**14**) so that only an abbreviated summary need be given here. Although Bernstein (**1**) reported 'alpha' conditioning of the eyelid reflex to sound, Hilgard and Biel (**8**) and Weber and Wendt (**14**) did not find satisfactory evidence for 'alpha' conditioning of the eyelid reflex to light. The former concluded that "'Alpha' conditioning, if any, has not cancelled the deleterious effects of negative adaptation." The latter investigators found some evidence for reflex augmentation in seven of their 48 *S*s, but they did not present statistical evidence of the reliability of these trends.

None of the above studies was designed specifically to take into account or to eliminate the effects of negative adaptation of the unconditioned reflex to light. It was apparent, therefore, that inclusion of an adaptation control procedure might result in a clear demonstration of 'alpha' conditioning even though the findings of prior studies had been ambiguous. The present experiment was designed accordingly. Two procedures, an orthodox conditioning procedure, and an adaptation control procedure, were used. The experiment was so designed that contrasting the eyelid reflexes obtained under the two procedures would show the degree of 'alpha' conditioning in spite of the extent of negative adaptation effects.

Apparatus and Procedure

The recording and stimulating apparatus were essentially the same as those of a previous experiment on the eylid response (**6**).

The conditioned stimulus was an increase in brightness of a circular milk-glass screen (diameter 10 cm.) from about .01 millilamberts to about 182 millilamberts, as measured by a Mac-

beth Illuminometer. The milk-glass screen was situated 60 cm. directly in front of the *S*'s eyes. The duration of the light stimulus was about 750 ms. *Ss* were instructed to maintain constant binocular fixation on a small black dot in the center of the milk-glass screen, except during rest periods. The unconditioned stimulus was a puff of air to the right cornea. When both the conditioned and unconditioned stimuli were presented on the same trial, the puff was delivered about 400 ms. following the onset of the light. Trials were given at random intervals ranging from 25 to 45 sec.

The right eyelid was connected by a thread to a simple lever system which amplified the extent of the lid movements threefold with relatively little mechanical distortion. Recording was done by means of a modification of the Dodge photochronograph (2).

The plan of the experiment was simple. Two groups of *Ss* were used, an experimental or 'alpha' conditioning group of 32 *Ss*, and an adaptation control group of 29 *Ss*. The two groups received a pre-test, a reinforcement or adaptation series, and a post-test. For both groups, the pre-test and post-test each consisted of four trials with the light stimulus alone. The reinforcement series for the 'alpha' conditioning group consisted of 40 trials on which the light and the puff stimuli were presented together. The adaptation series for the adaptation control group consisted of 40 trials on which the light was presented alone. *Thus the only difference between procedures was the inclusion of the unconditioned stimulus in paired relation with the conditioned stimulus in the 'alpha' conditioning group.*

Control records were obtained in the middle of each pre-test and post-test to check on the possibility that the *Ss* might be responding to incidental cues from the apparatus or experimenter. Short rest periods were inserted following the tenth and thirtieth trials of the reinforcement and adaptation series. Ready signals were not given during the experiment.

The *Ss* were men and women students from elementary courses in psychology who were asked to volunteer. They were assigned alternately to the experimental and control groups according to the order of their reporting for the experiment.

Results

Examination of the latencies of all eyelid responses recorded on the pre-test and post-test revealed a well-defined distribution of reflex closure latencies ranging from 50 to 100 ms. with a mode at 70 ms. All responses within these limits were considered to be unconditioned reflexes to light.

As indicated earlier, the experiment was so designed that a comparison of the light reflexes of the experimental group with those of the control group would disclose the existence of any reflex sensitization in spite of adaptation effects. 'Alpha' conditioning would be demonstrated if the reflexes of the 'alpha' conditioning group were more frequent or of greater amplitude than the reflexes of the adaptation control group—provided, of course, that the differences were statistically reliable.

Differences in Reflex Frequency: The mean frequencies of eyelid reflexes to the light stimulus on the pre-test and post-test and during the reinforcement and adaptation series are given in Table I with the corresponding SD's. The 40-trial reinforcement and adaptation series are divided into successive 10-trial blocks for more detailed analysis. Frequency means and SD's have been converted to percentages in order to facilitate comparisons. The F-ratios for the inter-group differences as computed from covariance analyses (see below) also appear in Table I. The confidence level for the signifi-

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TABLE I

MEANS AND STANDARD DEVIATIONS OF THE PERCENTAGE FREQUENCY OF EYELID REFLEXES ON PRE-TEST, POST-TEST, AND DURING REINFORCEMENT AND ADAPTATION SERIES

Group		Pre-test	Reinforcement and Adaptation Series Trials					Post-test
			1–10	11–20	21–30	31–40	Total	
'Alpha' Conditioning	M	38	52	46	46	43	47	43
	SD	34	31	38	39	37	31	36
Adaptation Control	M	28	17	18	17	16	17	23
	SD	30	25	23	26	22	22	28
$F = \frac{\sigma^2_{between}}{\sigma^2_{within}}$		1.92	9.05	20.57	14.21	13.10	29.05	5.84
Confidence level of the difference		20%	1%	0.1%	0.1%	0.1%	0.1%	2.5%

cance of each difference between the experimental and control groups is given in the last row of the table. The averages of Table I are presented graphically in Fig. 1.

Table I and Fig. 1 show that the frequency of reflex responses to light was consistently greater in the 'alpha' conditioning group than in the adaptation control group. The frequency differences appear greatest during the reinforcement and adaptation series.

The pre-experimental difference in reflex frequency was 10 percent in favor of the 'alpha' conditioning group. This difference, however, was not reliable, since the F-test or *z*-test (**4**, p. 234 ff.) showed that the probability of such a difference materializing because of chance factors was about .17. Nevertheless, it was thought advisable to balance out the initial difference in evaluating later differences in frequency. This was conveniently accomplished by the use of analysis of covariance, which not only took into account initial differences in central tendency but also eliminated variability due to individual differences (cf. **11**, p. 180 ff.). The analyses were performed separately for frequency of reflexes during each 10-trial block, the total frequency during adaptation and reinforcement series, and the frequency on the post-test. Raw frequencies rather than percentage frequencies were used in these computations.[1] Thus, any given significance level appearing in Table I indicates the probability of the corresponding inter-group difference in frequency being

[1] Statistical note: Reflex frequency in 4 ,10, or 40 trials blocks could hardly be distributed normally in the population (cf. means and SD's of Table I). It has been shown, however, that the F-test is not too seriously affected by lack of normality in the population (**3**, **11**), particularly when the number of degrees of freedom for the denominator variance is large (**13**) as in the present case.

a 'chance' difference, in view of the initial differences in central tendency between the groups. For example, during trials 11–20 of the reinforcement series, the average percentage of reflexes was 46 in the 'alpha' conditioning group and 18 in the adaptation control group. After balancing out initial (pre-test) differences, the difference between these averages was significant at the 0.1 percent confidence level. The probability levels were determined from the tables of Fisher and Yates (5) and the tables of Merrington and Thompson (12).

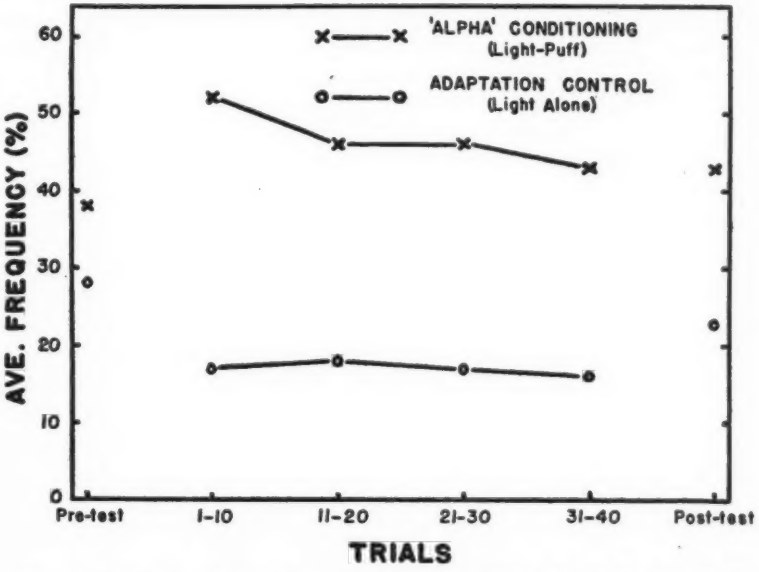

FIG. 1. The mean percentage frequency of eyelid reflexes evoked by the light stimulus on Pre-test, Post-test, and during the reinforcement and adaptation series.

All frequency differences after the pre-test were significant at confidence levels lower than one percent, except for the post-test difference which was significant at the 2.5 percent level. The differences were all in favor of the 'alpha' conditioning group.

Differences in Reflex Amplitude: Reflex amplitude was averaged for each *S* in such a way as to avoid spurious correlation with frequency; i.e., absence of response was *not* counted as a response of zero amplitude (10). The average amplitudes of the reflexes of the 'alpha' conditioning group and the adaptation control group were practically identical on the pre-test at the beginning of the experiment. For the remainder of the experiment, the average amplitude

of the reflexes of the 'alpha' conditioning group was twice as great as the average amplitude of the adaptation group reflexes. The differences between groups, although they were in accord with the trend of frequency differences, at no time approached statistical significance.

Adaptation of the Eyelid Reflex: The rapid adaptation of the reflex to light which seems usually to occur (**7, 8**) is not striking in the present experiment. The decrement (significant at the five percent confidence level or better) in reflex frequency which occurred in both groups (cf. Fig. 1) was, however, undoubtedly an adaptation phenomenon. The relatively high stimulus intensity in the present experiment can easily account for such differences in rate of adaptation as have been found between the present and prior studies.

Discussion

The highly reliable differences in reflex frequency favoring the experimental group constituted clear-cut evidence for 'alpha' conditioning. Under the conditions of the present experiment, 'alpha' conditioning manifested itself chiefly as a tendency for members of the experimental group to maintain a high frequency of reflex responses during reinforcement. In contrast, the reflex responses in the control group showed a distinct drop (significant at the two percent confidence level) in frequency during adaptation trials.

Reflex amplitude was not reliably affected by the reinforcement trials of the 'alpha' conditioning group. Under the conditions of the present investigation, therefore, 'alpha' conditioning was primarily a frequency phenomenon.

Reliable adaptation trends noted during the reinforcement trials of the experimental group suggested that in the eyelid response system reflex sensitization is probably only a temporary phase of the conditioning process. This conjecture is supported by the rather ambiguous previous findings with regard to the existence of 'alpha' conditioning.

'Alpha' Conditioning and Previous Findings: The results of the present experiment show that 'alpha' conditioning of the eyelid reflex to light, although it can be demonstrated, is not an obvious and striking phenomenon. This conclusion is in line with previous findings. The present results support Hilgard and Biel's (**8**, p. 230) contention that reflex sensitization ". . . is assuredly not the typical result of conditioning," but the outcome of the present study is in better accord with the more positive view of Weber and Wendt regarding the existence of 'alpha' conditioning in the eyelid situation. Differences between the findings of the present and prior studies seem to be primarily differences of degree which can easily be ac-

counted for in terms of differences in experimental design and conditions.

Significance of 'Alpha' Conditioning: The chief significance of a demonstration of 'alpha' conditioning lies in such light as it may throw on the nature of the general process of conditioning. Some of the implications of 'alpha' conditioning are fairly clear. The very existence of 'alpha' conditioning points to the widespread, although sometimes transitory, character of the readjustments of responses during conditioning. *It is apparent that under certain conditions even the short-latency reflexes participate in the general mobilization of the response system in the course of adjusting to the new stimulus sequences.* To be adequate any theory of conditioning or conditioning reinforcement must take this into account.

In the eyelid system, insofar as its reaction to conditioning reinforcement has been explored, the primary reflex to light does not become an important permanent part of the final conditioned response. This may or may not hold true of the reflex responses of other action systems. Further data are needed before generalizations are in order as to the full role played in conditioning by the shifts in the pattern of the unconditioned reflex to the conditioned stimulus.

Summary

In order to investigate the extent of 'alpha' conditioning, eyelid reflexes to light were recorded under two experimental conditions. In an 'Alpha' Conditioning Group, 32 *S*s were given a *pre-test* of four trials with the conditioned (light) stimulus alone, a *reinforcement series* of 40 trials on which the conditioned and unconditioned (corneal puff) stimuli were combined and a *post-test* of four trials with the light stimulus. In an Adaptation Control Group, 29 *S*s were used, and the procedure followed was the same as that of the experimental group except that, instead of 40 light-puff combinations, an *adaptation series* of 40 light stimuli was given. Comparison of the reflexes of the experimental and control groups would thus reveal the extent of 'alpha' conditioning in spite of negative adaptation effects.

The results of the experiment may be summarized as follows:

1. The frequency of eyelid reflexes in the 'alpha' conditioning group was greater than the frequency in the adaptation control group during the reinforcement and post-reinforcement stages of the experiment. The frequency differences were highly reliable even after initial (pre-test) differences were balanced out statistically by means of analysis of covariance. This finding constitutes clear-cut evidence for 'alpha' conditioning of the eyelid reflex to light.

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2. Although the inter-group differences in reflex amplitude followed the same trends as the frequency differences, none of the amplitude differences was significant statistically. Under the conditions of the present study, therefore, 'alpha' conditioning was chiefly a frequency phenomenon.

3. Adaptation effects were noted in the frequency of eyelid reflexes during conditioning reinforcement in the 'alpha' conditioning group. This indicates that reflex sensitization is probably only a temporary phase in eyelid conditioning.

4. The existence of 'alpha' conditioning shows that the general readjustment of the response system which takes place during conditioning is sufficiently deep-seated to involve even the primary, short-latency reflexes.

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THE EFFECT OF PRACTICE WITH AND WITHOUT REINFORCEMENT ON THE JUDGMENT OF AUDITORY NUMBER [1]

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Recent papers by Keller and Taubman (**13**), Plotkin (**17**), and Spragg (**20**) on the learning of International Morse Code have indicated a strong general tendency on the part of their *S*s to underestimate the number of short tones present in certain Morse Code signals. As these researches point out, this under-estimation is so serious as to suggest thorough research on the judgment of auditory numerousness. The present study was formulated with this in view.

Our work is derived from two main classes of experimental investigation: the study of the range of attention or 'span of apprehension,' and learning problems wherein the *E* gives knowledge of results to his *S*s (one kind of reinforcement). Our specific problem is to study the judgment of the number of short tones presented successively as it is affected by continued practice in the presence or absence of verbally-given reinforcement.

Most researches on the span of attention have concerned themselves with visually-presented stimuli (**5**, **15**, **16**, **21**, **23**). To the knowledge of the writer, only two studies have used *auditory* stimuli, as described herein. One of these (**7**) was actually a study of rhythm; the other (**8**) resembles most closely the present research.

The investigators mentioned above, as well as the writer, used series of *successive* auditory stimuli, a novel departure from the usual span of attention techniques. Most of the rates used by Hall and Jastrow, and Jerome, as well as all of those used here, were faster than 10 per sec. This latter speed precludes the use by the *S* of rhythmical grouping as an aid in the perception of the stimuli.[2]

Hall and Jastrow presented series of clicks to their observers. They used five rates of stimulus presentation, but report data on only two of these (intervals between clicks equal to .0895 and .0523 sec.). The stimuli were given in random order and from "three or four to ten observations on each number (more on the small than one the

[1] This problem was undertaken for an Advanced Class in Experimental Psychology conducted by Prof. R. S. Woodworth, assisted by Dr. E. A. Jerome. To these men, as well as to Prof. F. S. Keller, the author is indebted for advice and suggestions concerning this study.

[2] Woodworth (**25**, p. 695) points out that if the interval between successive sounds is less than a tenth of a sec., rhythmization is difficult or impossible.

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larger numbers) were made. . . . The effects of practice are obvious" (7, p. 58). The *S*s showed much under-estimation of the number of clicks sounded, and to explain this phenomenon Hall and Jastrow make the following statement (7, p. 60):

> "We may however conceive the earliest announcement of the impression of the first click in consciousness, and the exit therefrom of the registry-innervation involved in counting it, as separated in time by some not inconsiderable proportion of the simple reaction-time from ear to tongue. If the interval between the clicks is greater than or equal to this reduced reaction-interval, consciousness is done with the first click when the second arrives, and there is no error. If, however, the second click begins to be recognized in the focus of consciousness before this has completely initiated the act of tallying the first, and if the fastest rate of doing so has already been attained, then the third click will come a little later in the process, until at length a click in the later afferent state will cease to be distinguishable from the perhaps more widely irradiated process of the earlier efferent stage of tallying and will drop out of consciousness and be lost. . . . There is a disparateness between hearing clicks and counting, as there is between hearing the bell and seeing the index moving over the divisions of the dial, only it is of a different kind and perhaps degree; but the two acts are united in a 'complexion' (Wundt) like all other impressions, if their apperception is simultaneous."

Jerome, using short tones emitted by the apparatus outlined here, had six *S*s judge numbers from one to sixteen over a wide range of rates. He found that there was some slight over-estimation on numbers 1–3, but, in general, under-estimation was very prevalent and increased as a function both of number and rate of stimulus presentation. He also reports that, while the percent correct measure is an unreliable one, it decreases as a function of number and rate.

Our use of reinforcement as a variable is supported by much investigation, a review of which is manifestly beyond the scope of this report. We need state here only what seem to be the main contributions of reinforcement versus no reinforcement, as studied in the practice experiment.

Researches in a variety of fields (1, 4, 6, 9, 18, 24) have shown that reinforcement is more effective than no reinforcement in bringing about improvement. It has been held (11) that the *S* does not have *control* over the habit, and therefore modification is extremely difficult. Other investigations (22) have indicated that the unreinforced *S*s showed some modification of their behavior in the sense

that their variability decreased early and later scores were massed closely about the mean of the total trials. Finally, still other researches have suggested that there was actual improvement without reinforcement (**2**, **10**, **12**, **19**). Judd, in working with the Muller-Lyer illusion, states (**10**, p. 31):

> "The illusion disappears after practice. It disappears, not by any process of judgment or any process of indirect correction. The line comes to look differently than it did at first. . . . We have here, then, a change in the perceptual process, which change has taken place gradually through repeated efforts to deal directly with the object perceived."

This improvement, in cases without reinforcement, has been attributed to greater familiarity with the requirements of the experimental situation, a fuller comprehension of the discriminable characteristics of the stimulus, and, lastly, to a kind of personal reinforcement that might arise kinesthetically, for example, in shooting a bow and arrow (**14**) when the *S* 'feels' that his form and movements have been correct.

Apparatus and Procedure

The stimuli used in this experiment were short tones ("dots" in telegraphy), ranging in number from one to six; a stimulus of 10 dots was also used in an attempt to detract the observers from our main interest in stimuli 1–6, but the data are not reported on this stimulus. The tones were produced by a Zimmermann timing device, run by a constant speed A.C. motor (60 cycles) and connected to the output of a small Budd audio-oscillator with built-in amplifier. The pitch and frequency of the tones were set to the convenience of the *S*s at the first session and maintained throughout the experimental sessions.

The apparatus was arranged so that the arm of the Zimmermann timer would make a complete revolution in seven sec. Stimulus masks for the timer were designed to give three rates of stimulus presentation, intervals between two successive dots being equal to .1 sec. (rate 10), .0714 sec. (rate 14), and .056 sec. (rate 18).

One rate of stimulus presentation was used at a time, and the *S*s were allowed several minutes to become habituated to this rate before they began to observe. The order of the three rates for each day was random, as was the sequence of stimuli.

The basic pattern in the experiment was:

1. Stimulus.
2. *S*s' observation.
3. Empty interval (unreinforced group of *S*s) or verbal announcement by the *E* (three sec. from the cessation of stimulation) of the stimulus number just given (reinforced group).
4. Empty interval.
5. Next stimulus.

As mentioned above, the total time for steps 1–4 was seven sec.

Each set of *S*s met separately with the *E* in a small room free from reverberation and undue external noises for six consecutive days, and observed as a group, though the judgments were made and recorded individually by each *S* without consultation with other members of the group. In a day's trial of about an hour, 10 judgments were made for each stimulus at each of the three rates. Thus, at the end of the study, 60 judgments had been made for each stimulus at each of the rates.

The *S*s were told that this was a study to find out how well they could judge the number of dots that would be sounded. They were informed that, after a certain number of judgments,

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they would be given a rest and then the speed of the stimuli would be changed, though their task would remain the same. It was impressed upon them that what was called for was a judgment as to the number of dots they had heard, and not a judgment plus or minus an allowance for stimulus speed.

Subjects

Ss for this research were seven undergraduates in an elementary psychology class at Columbia College. Four *Ss* constituted the group with knowledge and three the group without knowledge. None had any previous experience with either the kind of observation required or with Morse Code. Their services were voluntary and were recruited for 'research bearing on the war effort.' Motivation was high, even in the unreinforced group, throughout the investigation.

Results

The data presented in this paper may be analyzed with respect to the following for both the group with knowledge and the group without knowledge:

1. The mean judged number obtained for each stimulus number of dots.
2. The mean percent correct for the judgments of each stimulus number.

The values above are given for each of the six successive practice trials (wherein n = 10) for each *S* at the three different rates of stimulus presentation which were used.[3]

3. Total mean judged number for each stimulus at the three rates.
4. Total mean percent correct for the judgments of each stimulus at the three rates.

Tables I and II give the data and Figs. 1 and 2 are the graphic representations.

TABLE I

The Mean Judged Number Based on 60 Judgments of Each Stimulus of Each of Four *Ss* in the Reinforced (Reinf.) Group and Three *Ss* in the Unreinforced (Unreinf.) Group

Stim.	Interval					
	.1 sec.		.0714 sec.		.056 sec.	
	Reinf.	Unreinf.	Reinf.	Unreinf.	Reinf.	Unreinf.
1	1.004	1.00	1.00	1.01	1.05	1.04
2	2.06	2.02	1.81	1.99	2.04	1.89
3	3.02	3.02	3.11	2.89	2.98	2.54
4	4.07	3.86	4.17	3.32	3.88	3.01
5	4.95	4.49	4.95	3.98	4.85	3.58
6	5.58	4.99	5.82	4.49	5.57	3.98

[3] These data were submitted with the original manuscript and are now on file in the Psychology Reading Room, Columbia University.

TABLE II

THE MEAN PERCENT CORRECT BASED ON 60 JUDGMENTS OF EACH STIMULUS OF EACH OF FOUR *Ss* IN THE REINFORCED (REINF.) GROUP AND THREE *Ss* IN THE UNREINFORCED (UNREINF.) GROUP

Stim.	Interval					
	.1 sec.		.0714 sec.		.056 sec.	
	Reinf.	Unreinf.	Reinf.	Unreinf.	Reinf.	Unreinf.
1	99.58	100	99.58	99.44	95.42	96.11
2	95.42	97.22	92.08	93.33	88.75	85.56
3	92.50	98.33	80.00	84.44	66.67	54.44
4	77.08	82.22	66.67	32.28	63.75	16.11
5	68.33	51.67	65.83	18.33	50.83	3.89
6	65.00	26.67	74.17	2.22	56.67	1.67

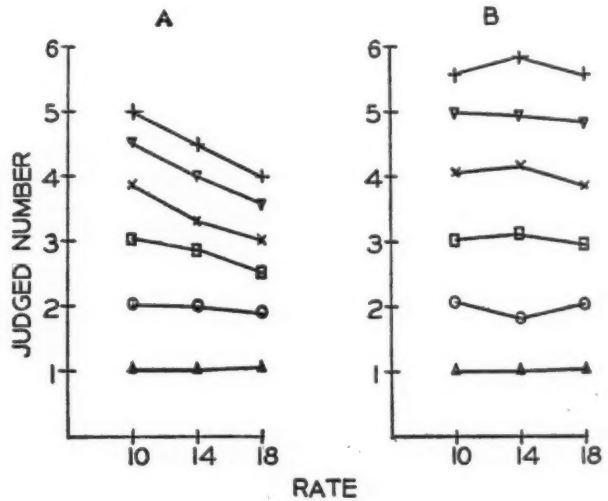

FIG. 1–A,B. A comparison of the total mean judged number as a function of rate, based on 60 judgments on each stimulus of each of three *Ss* in the unreinforced group (A) and four *Ss* in the reinforced group (B). The rates are 10 (.1 sec. interval), 14 (.0714 sec. interval), and 18 (.056 sec. interval).

Legend

Symbol	Stimulus
△	1
○	2
□	3
×	4
▽	5
+	6

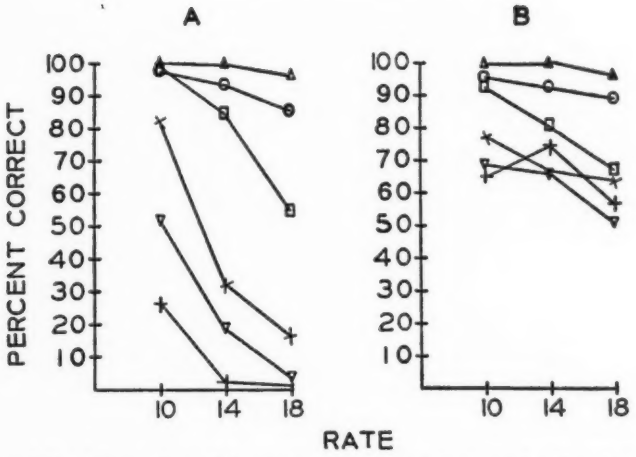

FIG. 2–A,B. A comparison of the total mean percent correct as a function of rate, based on 60 judgments on each stimulus of each of three *S*s in the unreinforced group (A) and four *S*s in the reinforced group (B). The rates and legend are described in Fig. 1.

DISCUSSION

The mean judged number gives the value ascribed to each stimulus, whatever the rate. It tells the direction of the error, if there is any, either in the form of over- or under-estimation, and also tells how large is this error.

If we examine the results in Table I and Fig. 1–A,B, we observe differences in the functions obtained from the reinforced and unreinforced groups. First, it will be seen that, for the reinforced *S*s, the stimulus scale maintains its integrity, while the scale for the unreinforced group evidences much compression as a function of speed of stimulus presentation and the stimulus number. This is to say that the difference between five dots and four dots, for example, is approximately one for the reinforced group and remains so with them with the increase in rate. With the unreinforced *S*s, however, the difference between five dots and four becomes smaller as a function of rate. And, further, it is the larger stimuli which, for the unreinforced group, suffer the most from this compression.

Second, as indicated by the day-to-day observations there seems to be a difference between both groups in the method of judging the number of dots presented. The *S*s in the group with knowledge proceed, in general, to over-estimate the number of dots. This happens even at stimuli 1, 2, and 3. This group over-estimates the small stimuli, and, with practice, reduces the estimate till it approaches the presented number. Certain instances of over-compensation for over-

estimation seem to be present. Finally, even at the end of practice, there is still over-estimation at the lower numbers.

The group without knowledge shows slight over-estimation for the smaller stimuli also, but these *S*s generally indicate that their judgments proceed from under-estimation to the gradual approach towards the actual stimulus number. Thus, there is a fundamental difference in the way the two groups of observers judge with practice. The reinforced *S*s over-estimate the stimuli and then attempt to reduce the value they ascribe to the presented number. The *S*s of the non-reinforced group under-estimate the stimuli and then increase the values they ascribe to the presented number.

At the higher numbers, 5 and 6, under-estimation is characteristic of both sets of *S*s at all speeds, but the group without knowledge manifests much more under-estimation. Both groups also attempt to pull up their judgments with practice.

The over-all mean judged number ($n = 60$ for each *S*) for each stimulus, at the three rates, gives a clear indication as to the most representative values given as a function of the number of dots presented at these various rates (cf. Table II). These data show that the *S*s of the reinforced group are over-estimating at all rates for numbers 1–4; only with stimuli 5 and 6 is there under-estimation. There is no evidence of regularity in the manner of over- or under-estimation. On the other hand, the trend is clear for the non-reinforced group. There is an apparently regular decrease in the number ascribed to the actual number presented, as we increase the rate.

From our study of the mean judged number, we may make the following generalizations:

1. Reinforcement is very effective in aiding the *S* to keep the stimuli presented disparate, and the difference between one stimulus and the next greater is approximately one; whereas, the absence of reinforcement results in a serious loss in differentiation of the stimuli, and this loss is progressive with greater rate and stimulus number.

2. Under-estimation is typical of all *S*s at the very outset. In the case of the reinforced group, these *S*s learn the direction of their error, and this results in over-estimation through over-compensation, and, to some degree, confusion.

3. With continued practice and reinforcement, our *S*s show a relatively greater vacillation about the true stimulus number. On the other hand, with practice and no reinforcement, the judgments of our *S*s start low and then approach the presented number; in this sense, they show greater learning. Our findings here are of value in pointing out that even in the absence of knowledge of results, our *S*s are groping with the material at hand and gradually getting better

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apprehension of the stimuli. Perhaps the under-lying mechanism involved is one of personal reinforcement, as discussed above. In any case, in this instance, absence of reinforcement seems to have superiority over reinforcement, for in the former situation, the *S* is apparently forced to struggle with the stimuli and make sense out of them—while with reinforcement, the *S* may take the situation too comfortably to wrestle with the stimuli.

So far, our statement of results has included only such information as is relevant to the numerical values attached by our *S*s to the stimuli presented. Yet, this information is inadequate insofar as we do not know the extent to which the *S*s judged correctly the stimulus number actually given them. These data are listed as 'percent correct' for every practice trial (wherein n = 10 for each *S*) with each stimulus at each rate and for the total series of trials (wherein n = 60 for each *S*) with each stimulus at the three rates (Table II and Fig. 2–A,B). Thus, we have a further check on the performance of the *S*s in the two groups in terms of the degree of correctness of the judgments.

The differences between both groups of *S*s are very apparent in the above tables and figures. The latter show that the reinforced group is well up at the high end of the scale, as compared with the unreinforced *S*s. Stimuli 1 and 2 are exceptions. Thus, the indication is that the reinforced group is judging the stimulus number correctly more frequently than the unreinforced group. Hence, in general, the judgments of the reinforced group conform more closely with the stimulus values.

For both sets of *S*s, the percent correct data given for the successive six practice sessions show many irregularities. Thus, no clear general statement can be made regarding the functioning of our *S*s with practice, as indicated by the percent correct that they achieve for all the stimuli at a given rate. Finally, the fact is evidenced that the percent correct decreases as a function of greater speed of stimulus presentation, and this holds true for all *S*s. But the percent correct plots for the group without knowledge descend more quickly than those of the group with knowledge, as the stimulus rate is increased.

Summary and Conclusions

An experiment was performed with two groups of *S*s on the judgment of short tones presented successively as it is affected by continued practice in the presence or absence of verbally-given reinforcement. The number of tones ranged from one to six, and three rates of stimulus presentation were used.

The data indicate a superiority of the reinforced group over the unreinforced group of *S*s, as measured by two indices: the mean

judged number and the mean percent correct. However, in many instances there is evidence for relatively greater improvement, as a function of practice, by the group without knowledge, and a principle of personal reinforcement has been postulated as the mechanism underlying this improvement.

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SIMPLE REACTION TIME TO CHANGE AS A SUBSTITUTE FOR THE DISJUNCTIVE REACTION[1]

BY ALBERTA STEINMAN AND SEYMOUR VENIAR

Introduction

The disjunctive reaction time has proved to be a serviceable tool for investigating the time required to perceive a difference between stimuli. The larger this difference the more rapidly the discriminative reaction takes place. On the basis of this finding Cattell (1) suggested reaction time as a method of measuring the difficulty of discrimination. The more difficult discriminations would require more time while differences which were equally difficult to perceive should give equal reaction times.

Pursuing this line of investigation Henmon (3) found an inverse relation between the discriminative reaction time and the objective difference between two colors, two tones, or two linear extents. He showed that reaction time can be profitably used as a differential index of the effect of supraliminal differences. Even beyond the transition zone, the increase of a difference brings still further decrease in reaction time. Adopting Cattell's point of view, and extending Weber's Law to supraliminal differences, Henmon reasoned that equal relative differences should require the same reaction time. Although his findings showed the mean reaction time to equal relative differences to be approximately constant, there was some decrease in reaction time as the magnitude of the total stimulus increased. Discrimination improved somewhat as the stimulus magnitude increased, as indeed is found also with the other psychophysical methods.

The purpose of the present experiment is to examine the availability of the *simple* reaction time for the study of discrimination. In a sense, every simple reaction is a reaction to *change* and involves discrimination, since any stimulus amounts to a change from the preexisting state of affairs, for example a change from zero intensity to the intensity of the given stimulus. Now, if the *O* is instructed to react to a sudden increase or decrease in the intensity of a light, his reaction to this *instantaneous* change should have the charac-

[1] This experiment was performed at Columbia University in the fall of 1942 as a project for Professor Robert S. Woodworth's course in Advanced Experimental Psychology. The authors wish to express their gratitude to Professor Woodworth, who suggested the method, for his generous counsel, and to Dr. Edward A. Jerome for invaluable assistance.

teristics of a simple reaction. There is a single movement to be made to a single stimulus. The disjunctive reaction demands a more complex and necessarily less complete preparation, and the disjunctive reaction time depends on the varying degree of this complex preparation as well as on the difference to be perceived. Hence, the disjunctive reaction time is not only slower but also more variable than the simple reaction time. Our question here is whether the reaction time to change possesses the advantages of the simple reaction time technique, and whether it is available as a substitute for the disjunctive reaction time.

As a matter of fact, the simple reaction to change has been used by Piéron (**5**) for a similar purpose, and it has recently been used in this laboratory by Flynn (**2**). The two investigators mentioned have not concerned themselves especially with the method itself, as is done in the present paper.

Further interest in the method derives from a suggestion of Jenkins (**4**). Having measured the simple reaction time to onset or cessation of a visual stimulus, and finding the reactions to cessation more rapid than to onset, he surmised that the disparity was due to a better preparation for cessation because the *O* was provided with a definite fixation point—the stimulus itself—whereas in the case of onset a fixation point was lacking. He suggested that equal opportunities for fixation and adjustment would be afforded by having the stimulus light continuous but changing in intensity at certain moments, with the *O* reacting to the changes in intensity. The RT might well prove to be the same for equal increments and decrements of intensity. We have followed this suggestion of Jenkins.

Apparatus and Procedure

Seated in a dimly lighted room, *O* saw before him a circular exposure plate which at six-second intervals was illuminated by a light. The light remained steady for 1200 ms. (the fore-period) and then was increased or decreased in intensity. *O* reacted to the change in intensity by raising his hand from a reaction key.

A modified Thorne (**6**) apparatus delivered the stimuli. The exposure plate consisting of milk glass, one in. in diameter, was in one end of a casing which contained two neon bulbs: the Standard (S), as we may call it, fixed in position; and the Variable (V), adjustable along the full length of the six-foot casing. The electrical onset of V, following 1200 ms. after the onset of S, provided an increment of intensity, which itself remained exposed for 1200 ms.; a decrement of equal duration was produced by the electrical cessation of V, 1200 ms. after the simultaneous presentation of S and V.

The magnitude of the changes in intensity depended on the distance of V from the exposure plate. Calibrations were made with the Macbeth Illuminometer.

Wratten neutral tint filters inserted on front of the stimulus plate provided two levels of stimulation, one of which was ten times as great as the other.

RT was measured by means of a Springfield Timer which started running at the onset or removal of V and was stopped by releasing the reaction key. The light circuit and the clock circuit were controlled from a rotating disk driven by a constant speed motor. These controls were located in a room separate from that in which *O* was seated, and every care was taken to eliminate sound cues.

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Nine settings of V were chosen to include large changes always perceived, and some changes so small as to be in the transition zone. A change of constant size was ordinarily presented 30 times in succession. After 30 presentations *O* rested, and a new value, selected at random, was set up on the apparatus. At a given experimental session, either the direction of change, or the level of intensity was varied, not both. In the first case, increments and decrements, in the second, reduction filters were rotated to equalize as far as possible the practice and fatigue effects under each of the conditions.

Three practiced *O*s served in the experiment.

Results

Reaction time to nine magnitudes of change at each of two levels of intensity is shown in Tables I and II. The data indicate that RT

TABLE I

Median Reaction Time in ms. to Relative Magnitudes of Change at Two Levels of Intensity

Level I = 2.06 C/M²; Level II = .206 C/M².

Δ I/I	*O*/A, Increments		*O*/B, Decrements		*O*/C, Decrements	
	Level I	II	I	II	I	II
.325	242	263	283	288	251	264
.192	243	267	281	296	269	290
.092	251	270	286	316	290	273
.047	260	263	312	356	278	272
.035	268	287	310	359	294	305
.027	275	306	330	383	298	313
.018	307	365	401	447	383	428
.013	356	380	442	471	443	474

TABLE II

Median Reaction Time in ms. to Relative Magnitudes of Change for Both Increments and Decrements of Intensity

Δ I/I	*O*/A, Level I		*O*/B, Level I		*O*/C, Level II	
	Inc.	Dec.	Inc.	Dec.	Inc.	Dec.
.325	231	200	258	205	247	243
.192	240	203	277	244	262	243
.092	236	199	283	260	273	248
.047	245	194	300	273	277	274
.035	286	230	280	267	299	296
.027	259	240	348	285	360	317
.018	305	288	410	339	440	346
.013	349	382	436	365	433	420
.012	—	—	470	420	515	485

to a change is shorter than the disjunctive RT. To respond to a relative difference of 30 percent in the length of lines, the quicker of Henmon's (**3**) *O*s required an average of 296 ms.; an average RT as low as 220 ms. to a relative change of brightness was obtained in the

present experiment. Further work now in progress with this method indicates that simple RT to change continues to decrease with increase in amount of change beyond the range here explored. RT to a change of sufficient magnitude is as short as the typical simple RT.

With regard to variability, RT to a change in visual intensity appears to be no more variable than simple reaction to onset or cessation of a visual stimulus. Jenkins (4) reports the standard deviations (SDs) of 400 simple reactions to the cessation of a light. For each of two *O*s, the first having had some training in RT experiments, the second having had none, the SDs were respectively 36 and 63 ms. Table III shows the SDs for each of the distributions of

TABLE III

THE MEAN REACTION TIME FOR EACH MAGNITUDE OF CHANGE, TOGETHER WITH THE STANDARD DEVIATION AND STANDARD ERROR OF THE MEAN

Data from *O*/C. Medians given in last column of Table II.

Δ I/I	Mean RT	SD	SE_M
.325	239	14	2.6
.192	242	13	2.4
.092	246	15	2.7
.047	270	27	4.9
.035	298	26	4.7
.027	309	26	4.7
.018	346	31	5.6
.013	402	49	8.9
.012	482	45	8.2

30 reactions made by one of our *O*s to changes of various magnitudes. The amount of variability appears to be related to the magnitude of the change. For the larger changes the SDs are even smaller than those reported by Jenkins for a decidedly supraliminal stimulus. Where the SDs shown in Table III are larger than 31 ms. they are associated with liminal increments.

The MVs reported by Henmon (3) for his experiments with the disjunctive reaction may be converted into SDs by the formula, SD = 1.25 MV. These converted SDs are found to range from 25 to 41 ms. for differences of 5 percent to 30 percent. Over the range common to Henmon's and our experiment, the SDs yielded by the simple RT technique vary from only 13 to 27 ms. The results clearly indicate that smaller SDs can be obtained with simple RT to change than with the disjunctive reaction.

Turning now to an examination of the relation between simple, RT to change and the amount of change, Tables I, II, and III show that RT becomes longer as the change in intensity decreases. This inverse relation holds for both levels of stimulation, regardless of

whether *O* was responding to increments or decrements. When RT is plotted against the stimulus ratio (Fig. 1), the best fit to the points appears to be obtained by a hyperbolic curve. This result is in agreement with that obtained by Henmon using disjunctive RT. Henmon's results for linear magnitudes have been plotted on this graph, for in spite of the difference in psychophysical materials, the range of stimulus values with which he worked coincides with that of the present study. Plotting the results of the two studies in this way makes the difference in the length of the simple RT to change and the disjunctive RT clearly discernible, besides illustrating the

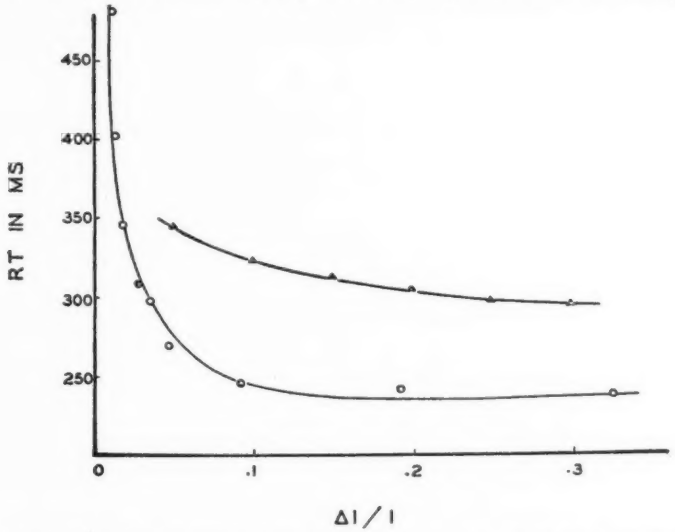

FIG. 1. Mean reaction time as a function of △I/I. The circles are for our *O*, data from Table III. Henmon's data with the disjunctive RT are indicated by the triangles.

similarity of the two forms of RT with respect to their relation to the stimulus variable. Since most of the changes in brightness employed in this experiment were supraliminal, simple RT to a change, as well as the disjunctive reaction, can be used to indicate the difficulty of discrimination with differences beyond the reach of most psychophysical methods. A limit to the decrease in RT with amount of stimulus-change is indicated by the data; there is a point beyond which RT remains approximately constant. In Table III the standard errors of the means are given to facilitate the calculation of the significance of the differences between means.

A further parallel to the disjunctive reaction may be noted in the way in which simple RT to change varies with the intensity of

the stimulus. In general, RT to change at the higher level is shorter than RT to the same relative change at the lower level of intensity. This disparity in RT is small but fairly consistent for all stimulus ratios and for all *O*s. The *t*-test of significance of the mean difference between RT to the stronger stimulus intensity and RT to the weaker intensity was applied on the assumption of the null hypothesis of zero mean difference. For *O*/A and *O*/B the value of P is below the .01 level, while for *O*/C the P-value is between .10 and .05. This finding that increasing intensity brings a reduction in RT agrees with Henmon's evidence for the decrease in the disjunctive RT as a function of stimulus magnitude. Results obtained with other psychophysical methods usually show a slight increase in the perceptibility of a constant stimulus ratio with an increase in the stimulus magnitude.

That the RT to increments is consistently longer than the RT to decrements is revealed by a comparison of the entries in Table II. For every *O* and for every degree of change, with but one exception, the reactions to decrements are faster. The mean difference between RT to increments and RT to decrements yields a P-value below the .01 level for *O*/B, and is significant at the .05 level for *O*/A and *O*/C. This result is analogous to the finding of Jenkins (4) that RT to cessation is faster than RT to the onset of a stimulus. The disparity in the opportunities for fixation to which Jenkins attributed the difference between RT to onset and RT to cessation cannot have operated to produce differences in RT in the present experiment, since a fixation field was provided by the standard regardless of the direction of the change in stimulation.

Conclusions

The value of the method of simple RT to change as a substitute for the disjunctive RT derives from its possession of the advantages of the typical simple RT. Requiring a less complex adjustment than the disjunctive reaction, the simple RT to change results in less variability and much greater speed.

A study of the relation between simple RT to change and the amount of change yields results which agree with those obtained by the disjunctive reaction. RT was found to be inversely related to the magnitude of change in brightness within and even beyond the transition zone, regardless of the direction of change.

With this method, data relevant to Weber's Law were secured. Faster reactions to a constant stimulus ratio were obtained at the stronger of two levels of visual intensity, a result which is not inconsistent with evidence on the factor of stimulus intensity shown by the disjunctive reaction and other psychophysical methods.

RT to decrements as well as to increments of brightness was studied, in an attempt to clear up a question on RT to onset and cessation. While the present method permitted equal fixation opportunities for both directions of change, reactions were significantly faster to decrements than to increments.

It may be concluded that simple RT as a measure of the response of the organism to a change makes possible the study of discrimination under the conditions of the typical RT experiment, and affords information as useful as that supplied by the disjunctive RT.

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EFFECT OF NON-RATIONAL FACTORS ON INDUCTIVE REASONING

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The careful scientist prides himself upon the skepticism with which he regards any hypothesis, whereas the ordinary man who has little insight into the pitfalls of hasty generalizations will accept at face value the most obvious explanation of a situation and may resent any suggestion that his interpretation is incorrect. A vivid experience is sufficient grounds for implicit belief for many persons. Most students probably fall somewhere between these two extremes. One has but to listen in on an ordinary 'hash session' to become keenly aware that many of the propositions which are supported were not reached by careful observations, and were not checked against adequate controls; nor do the debaters examine with much care the reliability of the logical arguments brought up to support the expressed beliefs.

It has long been known that, given an intense desire to believe a certain proposition, it is relatively easy to find some evidence to support it; consequently it has come to be a commonly accepted dictum that adherence to an erroneous belief is evidence of the desire to believe. While this may be so, the writer suspects that many false beliefs have their beginning in very trivial circumstances, and the experiment to be reported in this paper is merely an attempt to determine whether this suspicion may be verified. Our question is: may a generalization be accepted because of some entirely impersonal, non-emotional, and trivial circumstance?

If a person has been led to accept some generalization, it is conceivable that he might later be found supporting it with some vigor merely because it was his first interpretation. That is to say, a person might be led to adopt an hypothesis by a trivial circumstance and then led into championing that hypothesis as a means of being consistent. However, in this experiment we are not planning to measure any tendency to adhere to a generalization. We merely wish to determine whether a trivial circumstance may be instrumental in directing the *S* into adopting some hypothesis.

In the attempt to gain some experimental information upon this general problem, the following specific question was formulated: Given two equally tenable conclusions to a problem, can the ob-

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server's response be influenced by some unemotional, trivial, or irrelevant factor?

Description of the Experiment

The experimental situation was built with certain definite requirements in mind:

1. The material used must be of such a nature that no personal bias would be aroused in the *Ss*.

2. The setting must conform to the usual plan of inductive reasoning; namely, a certain phenomenon would occur after the operation of certain possible causal factors, and the combinations would change with repeated performances. The observer would be required to indicate which of these causal factors were essential to the result which he observed.

3. Equally tenable hypotheses would be open to the *S*, but under circumstances where one would be favored by some irrelevant factor.

In carrying out these three requirements, it was decided to use geometric designs as materials and to study the effect upon hypothesis-formation of such factors as the position, size, and simplicity of design of these figures.

The forms chosen were squares, circles, triangles, and diamonds. The favored position chosen was the left side of the stimulus pattern; the size-difference was to be great enough to be striking; and simplicity of design consisted merely in having an open figure in contrast with one complicated by a vertical line through the center.

In each situation, the favored and unfavored stimulus was to be present to the same degree as the favored one so that, logically, either one could be chosen.

The test as worked out was as follows:

Opening the Magic Door

This is a game whose object it is to find which one of four words will open a magic door. The words from which you may choose will be: square, circle, triangle, and diamond.

First, let us describe the way in which this door behaves.

It will open when the correct word is spoken to it. It can be made to open to different words; but once it is set, it will respond only to the word to which it is set.

It will also open when the correct lever is pressed. But the lever which opens it must carry on it a symbol which corresponds to the word to which it is set to open. Thus, when it is set to open when the word 'square' is spoken to it, it will also open when a lever bearing the symbol of a square is pressed.

Now, let us describe the game to be played.

You will see no actual door nor any actual levers. Instead, you will use your imagination. When you see a symbol on the pages to follow, you will imagine that symbol to be on a lever. From observing the imaginary opening of the door by the pressure on these imaginary levers you will get your cue as to the word which will open the door.

There will be 12 games and your task will be to find the correct word for each game. For each game all four words will be listed and you are to rank them from 1 to 4 in the order of their importance as door openers in that specific game. The correct word will change from game to game.

Specifically, in each game the door will be opened three times by pressing simultaneously three levers. From these three openings of the door you are to decide the relative value of the four different words as door openers.

Put a 1 after the word you think *most* likely to cause the door to open; put a 2 after the word you think *next* likely to cause the door to open; put a 3 after the word you think *next* likely to cause the door to open; and put a 4 after the word you think *least* likely to cause the door to open.

If, after you have started to do the problems you are not clear as to the procedure, you may turn back and re-read the directions as often as you wish.

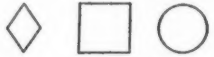
Results

The test was given to 246 students in psychology. Of these 246 students, six stated that two of the forms deserved equal weight in

Game 1. The door opens | Game 4. The door opens

Game 1. The door opens

when levers marked are pressed at the same time;

when levers marked  are pressed at the same time; and

when levers marked are pressed at the same time.

Square 1.83* Circle 2.99 Triangle 3.97 Diamond 1.21

*The numbers in the spaces left for rankings are the actual average rankings by 240 *Ss*. They are placed in the test form for easy reference.

Game 2. The door opens

when levers marked are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 1.73 Circle 1.32 Triangle 3.94 Diamond 3.00

Game 3. The door opens

when levers marked are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 3.01 Circle 1.71 Triangle 1.30 Diamond 3.97

Game 4. The door opens

when levers marked are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 1.12 Circle 1.89 Triangle 3.02 Diamond 3.97

Game 5. The door opens

when levers marked are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 3.99 Circle 3.00 Triangle 1.77 Diamond 1.24

Game 6. The door opens

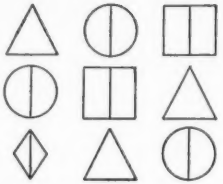
when levers marked are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 3.00 Circle 3.98 Triangle 1.14 Diamond 1.87

Game 7. The door opens

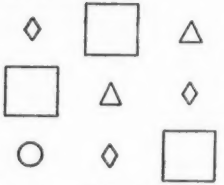
when levers marked  are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 3.00 Circle 1.66 Triangle 1.40 Diamond 3.98

Game 8. The door opens

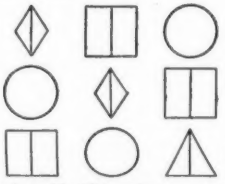
when levers marked  are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 1.29 Circle 3.99 Triangle 3.00 Diamond 1.73

Game 9. The door opens

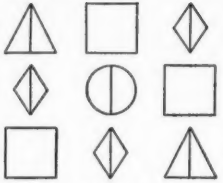
when levers marked  are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 1.36 Circle 1.66 Triangle 3.99 Diamond 2.95

Game 10. The door opens

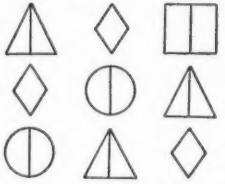
when levers marked  are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 1.25 Circle 3.96 Triangle 3.02 Diamond 1.77

Game 11. The door opens

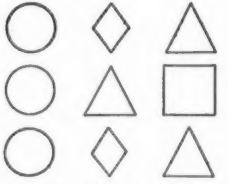
when levers marked  are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 3.98 Circle 3.01 Triangle 1.30 Diamond 1.71

Game 12. The door opens

when levers marked  are pressed at the same time;

when levers marked are pressed at the same time; and

when levers marked are pressed at the same time.

Square 3.96 Circle 1.16 Triangle 1.88 Diamond 3.00

each game and that no other forms deserved any rating whatsoever. That is to say, 2.5 percent of the total group refused to be sidetracked in their reasoning; they stated (and correctly) that rating these forms was an impossibility because two of them deserved equal weight and the other two deserved no consideration. Since these six could not be included in the tabulations to follow, it should be remembered that our statistical computations are based on the 97.5 percent of the *Ss*, i.e., those who did rate the various forms.

Of these 240 students, 10 stated that the two forms which appeared in all three openings of the magic door in each game deserved equal rank. In order to include their rankings in our tabulations, five of these were voted for the dominant form in each game and five for the non-dominant form. These 10 *Ss*, moreover, ranked the forms which did not always appear as belonging in third and fourth place and so they could be included without any modification.

After the adjustments made necessary by the responses of these 16 students were made, the average rankings were computed. They are given in the blank spaces in the test form itself and are summarized in Table I.

TABLE I

AVERAGE RANKINGS BY 240 STUDENTS OF THE 'LEVERS MARKED WITH VARIOUS GEOMETRICAL SYMBOLS.'

In the column to the left are indicated the number of times that the symbol appeared in the game for which the rankings are given and, in those cases where it appeared three times, whether it was dominant or non-dominant as to position, size, or simplicity of structure. The numbers in parentheses indicate the game in which the specific ranking was made.

Times Appearing and Characteristics	Form on the Levers			
	Square	Circle	Triangle	Diamond
Three times—Position dominant	1.12 (4)	1.16 (12)	1.14 (6)	1.21 (1)
Three times—Large size	1.29 (8)	1.32 (2)	1.30 (3)	1.24 (5)
Three times—Simple structure	1.25 (10)	1.66 (9)	1.40 (7)	1.71 (11)
Three times—Non-dominant position	1.83 (1)	1.89 (4)	1.88 (12)	1.87 (6)
Three times—Small size	1.73 (2)	1.71 (3)	1.77 (5)	1.73 (8)
Three times—Vertical line across face	1.36 (9)	1.66 (7)	1.30 (11)	1.77 (10)
Appeared twice in game	3.01 (3) 3.00 (6) 3.00 (7)	2.99 (1) 3.00 (5) 3.01 (11)	3.02 (4) 3.00 (8) 3.02 (10)	3.00 (2) 2.95 (9) 3.00 (12)
Appeared once in game	3.99 (5) 3.98 (11) 3.96 (12)	3.98 (6) 3.99 (8) 3.96 (10)	3.97 (1) 3.94 (2) 3.99 (9)	3.97 (3) 3.97 (4) 3.98 (7)

The most striking aspect of the rankings as exhibited in Table I is the tendency for our *S*s to be governed by the frequency of appearance of a particular symbol. Over 99 percent of the *S*s ranked the symbols appearing in all three of the door openings in each game in first or second place. This is the correct response, and the fact that only a fraction of one percent failed to place these in first or second position is an indication that the principle of the experiment was easy enough for all to grasp. The few errors were probably due to carelessness.

However, there is almost as strong a tendency to rank the symbols appearing but twice and once according to frequency of appearance. About 97 percent of our *S*s ranked the symbol appearing twice in third place, and about 98 percent ranked the symbol appearing but once in a game in fourth place.

This tendency to rank a factor in proportion to the frequency of its appearance demonstrates the ease with which a person can fall into an inductive fallacy. As long as a factor appears invariably in a causal group, the reasoner must concede that it may have some virtue as a causal element. When, on the other hand, the phenomenon occurs in the absence of a certain factor, that negative instance is enough to eliminate it from consideration. If two such factors have been shown to be irrelevant, why should the one which appears more frequently be given more weight than one which appears infrequently? A factor can be entirely useless in a causal situation even though it may present itself frequently.

To be sure, very few of our *S*s put symbols which did not appear in every opening of the door in a particular game in first or second place. In other words, they accepted the negative instances as evidence against the potency of the symbols which were not always present; but the fact remains that they rated one useless factor higher than another useless factor merely because it appeared more frequently than the less favored one. This finding makes on suspect that frequency of appearance plays too big a part in inductive reasoning. Although this experiment was not designed primarily to study the factor of frequency, our results certainly are clear-cut in this respect.

The three aspects which this experiment was designed to test were the effect of position, size, and simplicity of design on the formulation of hypotheses. We shall examine our results in these three areas separately.

The rankings of the different geometrical figures when in a dominant position were compared with the rankings of the same figures when in a non-dominant position, and these comparisons are set forth in Table II.

TABLE II

COMPARISON OF THE RANKINGS OF DIFFERENT GEOMETRICAL FIGURES WHEN IN A DOMINANT POSITION WITH THE RANKINGS OF THE SAME FIGURES WHEN IN A NON-DOMINANT POSITION

The numbers in parentheses indicate the game in which the specific ranking was made.

Position	Figures			
	Square	Circle	Triangle	Diamond
Dominant	1.12 (4)	1.16 (12)	1.14 (6)	1.21 (1)
Non-dominant	1.83 (1)	1.89 (4)	1.88 (12)	1.87 (6)

Source of Variance	Degrees of Freedom	Sum of Squares	Variance	F	P
Total	7	1.01475			
Between means of position	1	1.00820	1.00820	1592.7	<.01
Between means of forms	3	0.00465	0.00155	2.45	>.05
Interaction	3	0.00190	0.000633		

The variance between the means of forms is not significant. That between the means of position is highly significant. It is clear that, when it was possible to select one of two factors, both of which appear at every opening of the door and which are therefore equally acceptable as door-openers, our Ss tended to give first place to that form which occupied the first position in a column to the left, and to give a secondary ranking to another form which appeared all three times but in varying positions with successive door openings.

The ratings of the different geometrical figures when large in size were compared with the ratings of the same figures when smaller in size, and these comparisons are given in Table III.

TABLE III

COMPARISONS OF THE RANKINGS OF DIFFERENT FIGURES WHEN LARGE IN SIZE WITH RANKINGS OF THE SAME FIGURES WHEN SMALLER

The numbers in the parentheses indicate the game in which the specific ranking was made.

Size	Figures			
	Square	Circle	Triangle	Diamond
Large	1.29 (8)	1.32 (2)	1.30 (3)	1.24 (5)
Small	1.73 (2)	1.71 (3)	1.77 (5)	1.73 (8)

Source of variance	Degrees of Freedom	Sum of Squares	Variance	F	P
Total	7	0.4058875			
Between means of size	1	0.4005125	0.4005125	423.45	<.01
Between means of forms	3	0.0025375	0.0008458	0.90	>.05
Interaction	3	0.0028375	0.0009458		

The variance between the means of forms is not significant. That between the large and small figures is highly significant. This means that when two factors were equally qualified to be selected as the causal factor, our *Ss* tended to give preference to the larger form. However, size does not appear to be as effective in modifying rankings as does position.

The rankings of the different geometric figures when appearing in pure form were compared with the rankings of the same figures when they were complicated by the addition of a vertical line through their center. These comparisons are given in Table IV.

TABLE IV

COMPARISONS OF THE RANKING OF DIFFERENT FIGURES WHEN FREE FROM COMPLICATING LINES WITH RANKINGS OF THE SAME FIGURES WHEN COMPLICATED BY A VERTICAL LINE THROUGH THE CENTER

The numbers in parentheses indicate the game in which the specific ranking was made.

Complexity of Figure	Figures			
	Square	Circle	Triangle	Diamond
Simple figure	1.25 (10)	1.66 (9)	1.40 (7)	1.71 (11)
Complicated figure	1.36 (9)	1.66 (7)	1.30 (11)	1.77 (10)

Source of Variance	Degrees of Freedom	Sum of Squares	Variance	F	P
Total	7	0.2987875			
Between means of simplicity	1	0.000600	0.000600	0.15	>.05
Between means of forms	3	0.2859375	0.0953125	23.34	.05>P>.01
Interaction	3	0.122500	0.004083		

The variance between means of complexity is not significant. That between means of forms is significant at the five percent level. This means that our *Ss* were not influenced by the addition of a vertical line in their selection of the figure which caused the door to open. As a matter of fact, the square with the vertical line was given a slight preference over the one with the open face. The triangle with the open face was given a very slight preference over the one with the vertical line. The circle and diamond were practically tied. However, the general conclusion to be drawn from this table is that the degree of complexity introduced by a single vertical line did not play any significant role in the rankings of our *Ss*.

DISCUSSION

The experiment was drawn up with no preconceived notion as to which factor would dominate the rankings; the purpose was merely to equalize as nearly as possible all factors excepting the ones selected

for study. For example, when it was decided to test the influence of favored position it was thought to be possible that, instead of choosing the figure in the dominant position for favor, our *S*s might hunt for the least favored position on the ground that significant factors are often the hidden ones.

It is interesting to note, also, that one of the factors which we selected for study, namely complexity of form, did not prove to be significantly effective. Consequently, one should not generalize from this experiment and assert that position is a potent factor in inductive reasoning, with size a close second. Nor should one conclude that complexity of form is not a factor in hypothesis formation. We had no intention of generalizing in any such fashion from our results. All that can be said is that in this particular arrangement of factors, our *S*s did show that certain incidental trivia influenced their rankings.

In a carefully designed scientific experiment, care is taken to equalize irrelevant factors or the data are treated by statistical devices designed to locate and to allow for such features. This is another way of saying that scientists have long recognized the influence of chance and incidental elements in the outcome of their experiments. When they engage in research they must guard against such errors. Why then perform an experiment to show that such factors do operate? The purpose is to demonstrate that the average student does not conduct his thinking along scientific nor statistical lines. Our findings make it clear that he permits himself to be affected by irrelevant factors. Certainly, there is no more logical reason why a large figure or a uniformly placed figure should be more potent than a smaller one or an irregularly placed one; nor, as we have already pointed out, should the repeated presence of a factor which has been found to be irrelevant weight our judgment in its favor. Yet these factors all operated in the thinking of our students.

A conclusion may also be drawn from the fact that one of our trivia failed to function as a significant factor in distorting rankings. This fact should deter one from making *a priori* judgments as to what effect some feature of a situation will have upon the results obtained.

Summary

A very simple task was set up which conforms in pattern to ordinary inductive reasoning. The *S*s in the experiment were led to imagine a certain happening which was produced by certain factors in varying combinations. Symbols were used to represent these factors, and the student was required to generalize on the basis of repeated observations as to the possible potency of the various factors as causal agents in producing the observed phenomenon.

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The whole situation was made very concrete by supposing an imaginary door to open when certain levers, each designated by a geometrical figure, were pressed. The inductive process consisted of giving the names of the figures on the levers a relative ranking in the order of their effectiveness as door-openers. The whole exercise was in the form of 12 games, in each of which the door was opened three times by the pressures on a combination of three levers at each opening. Two levers appeared in all three combinations, making each logically equal as a first choice; another lever appeared twice in the three openings; and still another lever appeared but once in the three openings of the door.

Had logic alone prevailed in making judgments, the two levers which appeared each time the door opened should have tied for first place. Furthermore, if logic alone prevailed the same equal distribution should have been obtained between the levers which appeared once and twice in each triple opening. Since the door opened without their functioning in one or two instances, they should have been excluded with equal frequency from consideration.

Moreover, various irrelevant or incidental features were introduced to determine whether trivia are effective in influencing inductive generalizations or hypothesis formation. The following conclusions were obtained:

1. The highest ranking was given to those factors which appeared without exception in all three door openings of a series. The few exceptions (less than one percent of our *S*s) to this were probalby due to carelessness.

2. Frequency of the appearance of a factor was extremely effective in controlling the relative rankings of factors both of which were logically irrelevant.

3. Of two factors equally favored by logical considerations for first choice, preference was given to the one which held a dominant position in the experimental pattern or to the one which was larger than its rival for first place. Thus, our original hypothesis was supported, namely: given two equally tenable conclusions to a problem, the observer's response can be influenced by some unemotional, trivial, or irrelevant factors.

4. Simplicity of design played no significant role in determining relative rankings.

5. No favoritism was shown in the rankings for any one of the four geometrical figures used, namely, squares, circles, triangles, or diamonds.

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